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SUPPLEMENT TO]

[OCTOBER 17, 1900.

THE
CLINICAL JOURNAL

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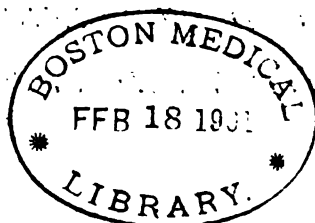
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*A Clinical Lecture on Indigestion.

By SIR T. LAUDER BRUNTON, M.D.,

F.R.S.

INDIGESTION.

A Clinical Lecture delivered at St. Bartholomew's Hospital, 15th December, 1899,

BY

SIR T. LAUDER BRUNTON, M.D., F.R.S.

*Clinical Studies. By W. JOSEPH TYSON,

M.D., M.R.C.P. 8

*Chapters from the Teaching of Dr.

G. V. POORE. No. XXXI 11

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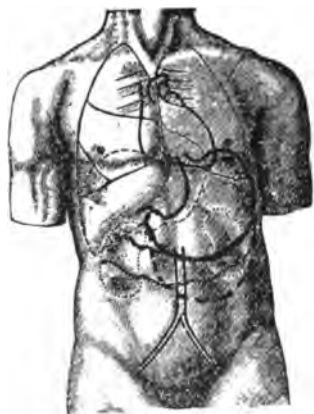
GENTLEMEN,—As this is the last clinical lecture of this year, so it is the last, not of the nineteenth century, but of the last year in which we shall use "18" in the number of the year. I have therefore thought that instead of simply taking a case in the wards as the subject of my discourse to-day I might take a subject of rather more general interest, one that will interest you very greatly between the time when we part to-day and the time when you next come to hear a clinical lecture at St. Bartholomew's Hospital, for during that interval you will have been partaking of those festivities that have for many centuries occurred at this season of the year in this country. For how many centuries they have taken place we do not know, because it was long before the Christian Era that feasts were held under the name of Yuletide feasts in honour of the year that was going out and of that which was coming in, when the shortening days of gloom and of darkness were just beginning to give place to the lengthening days of sunshine and warmth. When the Christian religion was introduced many of the old pagan feasts and ceremonies were adopted into it, and the festival that was at first intended to celebrate the coming of the new year was altered so as to represent the coming of the Child whose birth was to give to mankind a newer and better life. Both in Pagan and Christian ages these festivities have held an important place. They still do so, and they are to us a great landmark in the year, something which we look forward to with pleasure, and which we look back upon, in many cases at least, with satisfaction. But this satisfaction is occasionally tinged with a certain amount of,—shall I say, discomfort? for the very varied and tempting fare that we have at these Christmas festivities leads people occasionally

to indulge rather too freely in the pleasures of the table. Now it is with the object of telling you (1) why these discomforts come on, and (2) how you may best avoid or remove them, that I have taken the subject of Christmas festivities to-day.

Well, now, we have first to consider the amount of food that is taken at these feasts. There was once upon a time an American girl who was asked if she were hungry, and she replied, "No, I'm not hungry, but thank goodness I'm greedy;" and that is the feeling with which many people begin to partake of a Christmas dinner. The quantity that they get through is greatly increased by the variety of dishes, and mere quantity has a great deal to do with the discomfort that is felt afterwards. I have got here the menu of a dinner at St. Bartholomew's Hospital.

MENU.—Clear ox tail soup, thick hare soup, stewed eels and port wine sauce, boiled codfish and oyster sauce, fried smelts, ris de veau aux tomates, lark puddings à l'essence, braized turkeys, York ham and French salad, saddles of mutton, pheasants, seakale, gelées à la Brunswick, French pastry, compote of oranges, plum puddings, mince pies, ice puddings, and bloater roes on toast.

In addition to all that there were the drinkables. There was sherry with the soup, hock with the fish, champagne with the roast, a liqueur with the ice, and after that came claret, sherry, port, and liqueur brandy. Now you can think of the condition of the unfortunate stomach after all these things are put in.



Here is a diagram of the stomach where its outline, as it should normally be, is shown by a dark line. But after all these things have been put into it, it is probably very much bigger, as shown by the dotted

line. So that the mere fact that a considerably larger quantity of food is put into it than it was intended by nature to accommodate may have a good deal to do with possible indigestion afterwards. But it is not merely the quantity that is put in, it is also the variety of things which are put in. A number of those things whose names I read out to you, *e.g.* the salad, cannot be acted upon by the gastric juice at all. In all probability there was cheese, too, in that repast, which is very indigestible, because it is hard, and breaks into lumps which are easily swallowed without thorough mastication. Mince pies and plum puddings contain raisins, which are absolutely, or almost absolutely indigestible in the stomach and intestines. Supposing, then, that in the stomach there are lumps of various kinds, with a normal quantity of smaller particles of indigestible material, at the end of six or seven hours—the time during which an ordinary meal should have been passed from the stomach into the intestine—the overloaded stomach feels the need of emptying itself, and tries to do so. But the greater curvature of the stomach is much lower than usual, and the organ has an unusual burden to eject through the pylorus. The condition of the stomach is very like Mark Twain's celebrated jumping frog. This frog's name was Dan'l, and he "could get over more ground at one straddle than any animal of his breed." His owner, who kept him in a box, was very proud of the frog's jumping powers, and was always ready to bet on them. One day a stranger strolled in and inquired about the contents of the box. After a look at the frog he said, rather scornfully, "Well, I don't see any p'nts about that frog that's any better'n any other frog." This raised the indignation of the frog's owner, and he bet forty dollars that his frog would outjump any other frog in Calaveras County. The visitor replied, "I'm only a stranger here, and I haven't got a frog, but if I had a frog I'd bet you." "All right," said the owner, "if you'll hold my box a minute I'll get a frog for you." And off he went. But when the visitor was left alone he opened Dan'l's mouth, and with a teaspoon filled him full of shot nearly up to the chin, and set him on the floor. Then Dan'l's owner returned with another frog, and the two were put together with their fore-feet even. One, two, three, and at a touch from behind, the new frog hopped off, but Dan'l simply gave a heave and hoisted

up his shoulders, but he couldn't budge from the spot.

Now this is just what the over-filled stomach does. Instead of sending its digested contents into the duodenum it simply gives a heave and turns over. The lumps try to get through the pylorus, but they do not succeed; it will not let them through. In the meantime the alcohol in the stomach is undergoing fermentation, and so a quantity of vinegar is formed. By-and-by the stomach cannot stand it any longer; it is bound to get rid of its burden somehow, and the whole is accordingly ejected through the throat, after which the man is very much easier. But sometimes he does not get rid of the whole of it; much foul stuff is left behind, and he still has an uneasy sense of discomfort about his epigastrium. What is he to do? The best thing he can do is to drink a lot of water. It had best be lukewarm, because very hot or very cold water does not give rise to vomiting, while lukewarm water does, as you will find in the Revelation of St. John, where it says, "So then because thou art lukewarm, and neither cold nor hot, I will spue thee out of my mouth." Therefore you wash the stomach thoroughly out with lukewarm water. But even if you get the whole of it out, this stuff is so acid that it often sets the unfortunate person's teeth on edge—I can hardly call him a patient, perhaps I should call him the unfortunate sinner, as his sufferings are the result of his own fault. In order to neutralise this acid, which is smeared all over the mucous membrane, and which is still giving rise to irritation, you add bicarbonate of soda to the liquid. By that means you relieve the person very much. Sometimes vomiting does not take place spontaneously, and the old Romans, who were still greater gourmands than we are, found that their stomachs would not hold all that they wanted to put into them. So if you go to-day to the Palace of the Cæsars of Rome you will find a little place adjoining the banqueting hall where the Romans used to make themselves sick and empty their stomachs, so that they could go back to the hall and have some more. In this country, as a rule, when people have filled their stomachs too full they do not follow the Roman's example, and go back to the dining-room after emptying them, but like the Romans they sometimes seek relief by getting them emptied. And they probably attain their object

in the same way as the Romans, either by putting the finger down the throat, or by tickling the fauces with a feather. There is one point to be observed about the finger, and it is that unless the nail is cut very short it may scrape the throat so as to leave it very uncomfortable for a day or two afterwards. Therefore the nail should either be short, or the fauces should be tickled with something softer than the finger, such as a feather. A friend of mine told me that he was very liable to indigestion, and that it was his practice, immediately on returning from a dinner, to use the stomach pump and wash that organ thoroughly out. In this way he said he saved himself from a great deal of trouble and discomfort. But surely a much better plan than his is to prevent indigestion rather than cure it, and be content with a smaller quantity, and with less variety of food.

But supposing that indigestion has occurred, and that the gourmand has got rid of the excess of food, what is to be done with the mucous membrane of his stomach, which still remains irritated by the very acid substances formed by the decomposition of food and drink? Very often it happens that if you simply allow such patients to abstain from food altogether for a while, and thus give the stomach a rest, it recovers quickly without any other help. And fortunately the irritation of the gastric mucous membrane which succeeds to the ingestion of too much food and drink, leads to a condition of lack of appetite or even nausea, which prevents the patient from taking more. But if the patient should not feel this, it is wise for you to insist upon giving the stomach a little rest after the over-exertion to which it has been subjected. Let the patient take very little food for the next day, and that only of a very plain sort, such as a little tea and toast, and some boiled rice or Indian corn-flour, or something of that sort—a plain light diet. If this is not sufficient to remove nausea, you may give the patient some bismuth, bicarbonate of soda, spirit of chloroform, and cinnamon or peppermint water. By such means you will probably find that the stomach soon gets all right again. But sometimes, instead of the stomach retaining all the food, it allows some of it to pass into the intestine, and there the food, partially decomposed, and to a great extent unmasticated, acts as an irritant also, and fortunately produces diarrhoea, very frequently accompanied by griping. The gripes of course are

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CLINICAL STUDIES.

BY

W. JOSEPH TYSON, M.D., M.R.C.P.

Renal Asthma, but perhaps more correctly called Uræmic Dyspnœa.

ONE is so in the habit of associating shortness of breath, apart from anæmia, with cardio-vascular or respiratory change, that the kidney as a cause of dyspnœa is apt to be forgotten. I am not referring to those severe attacks of great difficulty of breathing which come on in the early part of the night, in which there is an agonising want of breath with great inspiratory effort. The cases I am bringing before you have been aptly called "continuous dyspnœa," and apparently the only sign that is noticeable on meeting your patient is shortness of breath, and it is the principal thing that he complains to you about. On examination of the chest one finds little or nothing wrong, except perhaps a slightly enlarged left ventricle. As a rule there is no dropsy present, and as far as I have seen convulsions have not yet come on.

It is difficult to understand why some cases should suffer with shortness of breath and others should not, nor does the amount of albumen present seem to play an important part.

Dr. Dickinson has suggested that spasm of the pulmonary artery is the *modus operandi* of the attacks; but I think that he must be referring to the paroxysmal attacks, and not to those I am now mentioning, for one can hardly think of the artery being more or less permanently contracted.

I should think that a very probable cause would be that some uneliminated renal product circulating through the respiratory nervous centre, and the resulting irritation produce an increase in the respiratory rhythm.

The practical outcome of the above remarks is this, that in all cases met with of shortness of breath, it is important to remember that the kidney may be the offending organ.

The Danger of putting Old People to Bed.

Probably I shall be running a-tilt against some-one's opinion when I say that I believe that many

old people are sent to bed too early, and are kept too long in bed when they are convalescing. Of course there are many obvious exceptions which will at once occur to you, and which I need not take up your time in mentioning.

I believe that we should be chary in advising old people to take to their beds when slightly ill. Some few years ago I was attending an old man of eighty with a slight attack of bronchitis, the like of which he had often suffered with before. A consultation was suggested, and the consultant called in advised my patient going to bed. I remonstrated, but gave way to what I thought then was wiser advice than my own. The patient kept his bed for a week and then died. It may have been a case of "post hoc," and not "propter hoc," still my experience has since led me to believe that this man would have had a better chance of recovery if he had remained up during the daytime.

We must remember that the will power of old people is comparatively weak, and that bed to them is comfortable; then when once in bed after a few days the heart and muscular system lose tone. The effort to overcome the above is difficult, and sometimes impossible to obtain again. It often requires a good deal of courage to keep these old people from taking to their beds, for the patient often takes kindly to bed himself, and the friends think you unkind in advising less rest.

My own belief, then, is that it is wise to keep old people about and with moderate exercise as long as possible.

Sensitive Skins of Children.

I was vividly reminded a few months back how sensitive many children's skins are to outward applications. A child aged 8 years was suffering from a bronchial attack, associated with very severe pain over the lower part of the sternum: it went on for two days with no relief from remedies ordered. On the third day I ordered warm fomentation, a teaspoonful of turpentine being added to half a pint of the warm water with which the flannel was soaked. Within a day or two there was considerable inflammation, and ultimately the skin of the chest sloughed away over an area of about 3 inches by 2 inches, and the sore left did not heal for five or six weeks.

I admit that the skin of the above child was

known to be sensitive to outward influences, but still it is important to remember the clinical fact above referred to, especially as in these days more and more is being done by nurses, constantly uncontrolled by the medical man in attendance, and I think before very long we shall have to consider whether we had better not exercise a little more supervision over the nurses under our care than we have been doing in the past.

Sudden Death in Old People.

All of us must have met with cases of sudden death in quite old people, and have wondered as to the cause of it. There is a prevalent opinion, perhaps more common among lay people than our own, that the aged folk gradually die, or in other words their last illness is rather lingering. Perhaps on the whole this is correct; but still there are a large number of comparatively healthy old people who depart this life suddenly and unexpectedly. I have always been in a difficulty about signing their death certificates, and have generally adopted the vague and unsatisfactory terms of "old age" and "syncope." I think there are two causes that not infrequently occasion death, viz. (1) displacement of a clot, and (2) a full meal taken just before going to bed, thus causing not only irritation of the pneumogastrics, and so slowing the heart's action, but also there is the mechanical distension of the stomach acting upon the heart, and the pressure upon it is aided by the prone position of the body. I have seen a death from this latter cause.

I have known of three sudden deaths in young people from causes not discoverable.

The Use of Chloride of Calcium in Hemorrhage.

The value of the above drug in hæmorrhage does not seem very well known. Many of the text-books do not even mention it, yet those who have tried it speak most highly of it. It is only within the last two or three years that it has been discovered how to make the blood more coagulable, and this knowledge has come about through the researches of Hammersten, Arthus, Wright, and others.

If you precipitate all the calcium salts from the blood, it is rendered incoagulable, — so chloride of calcium taken internally is prescribed.

In one case under my care of almost uncontrol-

lable hæmorrhage from the big bowel, due to extensive ulceration, chloride of calcium given in fifteen-grain doses, three times a day, had an almost immediate effect after everything else had failed. I have also ordered it with some success in severe prostatic hæmorrhage.

To the surgeon who fears bleeding during his operation the giving of lime beforehand is stated to be of great use.

Cases of High Temperature with no Discoverable Cause.

What a number of cases we meet with in the course of a few years with temperatures varying between 100° and 103° , without being able to find a cause! Some few months ago I had a series of cases in which the temperature varied between 100° and 102° , and continued for a week. I was unable to discover any local cause or general infection. These cases were recorded in the 'Medical Magazine.' I have come to the conclusion that very many of the above are best explained by absorption of poisonous material — whether bacterial or not I cannot say — evolved during gastric or rather intestinal digestion. The study of food-changes during its sojourn in the bowel is at present only in its infancy. "What is one man's meat is another man's poison" is absolutely true, and was long ago found out by practical experience before the present knowledge of chemistry or bacteriology was thought about. Our fathers and grandfathers understood almost better than we do the immense value of a blue pill or calomel followed by a black draught in cases of what they called, I believe rightly, gastric fever; that is to say, these cases were due to absorption by the stomach or the intestine of some offending material, which acting through the circulation on the heat centre set up fever, and the treatment, although no doubt empirical, was antiseptic and eliminant in action, and the patients said they were better for it.

There is no doubt that the heat centre can be disturbed in many ways which at present we do not understand, and I believe before very long moderate and high temperatures will be much more accurately gauged as to their intrinsic values. A high temperature may or may not be deleterious to the patient, and certainly it is not to be knocked down on every conceivable occasion. If we could always find out the cause of it, the treatment might

then become easy ; but now we have often to wait, and it is not unreasonable to suppose that in some instances the extra heat produced in the body is protective in its influence.

Simple Retention of Urine ; or, in other words, an inability to pass water from no assignable cause.

Two of these cases have come under my notice, and after a week of catheterism got well.

I could find nothing wrong with the bladder or urethra ; there was no history of chill or error of diet.

On examination there was no spasm noticeable in the urethra, as sometimes occurs from gonorrhœa or a drinking bout. The cause seems to be in the loss of tone of the bladder wall.

I have thought of a possible cause in the excessive acidity of the urine ; but perhaps some of you can help me out with a better explanation.

My treatment in both cases was by the administration of strychnia and 3ss doses of bicarbonate of potash in barley water night and morning.

Respiratory Exercises for the Promotion of Absorption of Pleuritic Effusion.

There has been a good deal written about respiratory exercises lately, but personally I have not come across any cases in which these exercises have been recommended or used to help to restore the compressed lung after pleuritic effusion.

Some four weeks ago the following case came under my care. A girl aged 15 was attacked with severe pains in the right side ; three days later pleuritic effusion to a slight extent was noticed ; the fluid gradually increased, and at the end of the week there was dulness over the lower two thirds of the chest ; no further increase took place. At the end of fourteen days diminution of fluid began to take place, and for the next week this was continued. It then occurred to me that if the lung could be helped to expand, not only would the lung itself be likely to improve, but also the fluid would probably disappear quicker. I ordered my patient to have respiratory exercises by a trained person three times a day ; and these have been continued for eight or nine days ; the result is that my patient is decidedly better, I will not say necessarily due to the exercises.

Perhaps it would be better to tap in all cases of pleuritic effusion, but somehow in private practice

one is apt to wait a little longer before tapping than one would do if the case were in hospital, and the reasons for waiting longer I think are obvious.

I believe one may tap a chest too soon, as well as too late—my own case was somewhere about the boundary line, and being in private I waited ; but I had some qualms of conscience about the lung getting too compressed, and so not being able to quite recover its full breathing power, and then the thought came into my mind of trying to dilate the lung if possible by exercises. I think the subject is worth thinking over ; and possibly this form of treatment may lead to useful results.

Keeping the side at rest by strapping has been strongly recommended in the early stage of pleurisy, and perhaps exercise of the side may prove equally successful in the later stage of the disease.

Resuscitation of apparently Dead New-born by Laborde's Method.—Laborde's method

of resuscitating the apparently dead, which consists in the rhythmical traction of the tongue by two fingers covered with ordinary cotton or a handkerchief, the traction being at the respiratory rate of 18 to 20 per minute in adults, a little more rapid in the new-born, has been successfully tried in personal cases. The nose and mouth should be cleared of mucus. As in all other methods of resuscitation of the new-born, one should not lose courage if after several minutes there are no results or signs of breathing. The tongue at first will give no resistance ; after a while it resists positively ; soon very mild respiratory movement is noticed, then all is quiet. In a short time the breathing is stronger and has a normal character, and the child begins to cry, move, etc.

Laborde's method is better than Schultze's, because the child does not become chilled, being all the time in a warm bath ; one can notice the beating of the heart ; the physician does not tire himself as easily as in Schultze's method, and can use it in cases in which the latter mode of resuscitation is impossible.—F. E. FRONCZAK, *Phila. Med. Journ.*, February 24th, 1900 ; *Monthly Cyclopaedia*, March, 1900.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXI.

GENTLEMEN,—The procreative power in a female extends normally over the menstrual period of life, and that period is very variable. It begins most frequently at fourteen, and then, in order of frequency, it begins at 15, 16, 13, 17, 12, 18, 19, 11, 20, 10, so that you have to accept the fact that menstruation may begin as early as ten or as late as twenty. Then you must remember that there have been on record cases of conception without any previous menstruation. Sometimes there may be great sexual precocity in women, just as there are instances of great precocity in men. Menstruation ceases generally between forty and fifty, perhaps at forty-five most often. But there are no hard and fast lines; it may be prolonged to fifty or sixty, or even to a greater age. I am now quoting cases which have been recorded as anomalous. Again, conception may occur after menstruation has apparently ceased.

There have been two decisions in the law courts which are of importance—two decisions which are both different, but both based upon sound common sense, and both, I take it, substantially correct. Vice-Chancellor Wickens, in the case of *Conduit v. Soane*, declined to regard it as an impossibility that two *married* ladies aged fifty-seven and fifty-two respectively could never have any children. Vice-Chancellor Kindersley, in the case *Forty v. Forty*, decided that an *unmarried* lady aged fifty-three might be presumed to be beyond child-bearing. By comparison of the two decisions you get a common-sense view of the matter under different circumstances. I am always warning you not to attempt to be precise where precision is impossible. You must remember that in regard to a woman the law does not concern itself with anything beyond the power to copulate. A woman when she marries is supposed by the law to be *habilis ad matrimonium*, fit for the marriage condition, and able to bear her part in the act of copulation, and if a divorce is sought for on sexual grounds the law only concerns itself with this phase of the matter. For instance, an obstetrician might declare that although there was a vagina the uterus was undeveloped, and the

woman could not bear any children; the law would not take cognizance of that, but if there was inability to copulate a divorce would be granted. There are cases on record of so-called atresia vaginæ, where the vagina is practically non-existent, and such cases have been pronounced suitable for divorce.

Now I come to something a little more definite and more important, namely, the question of *rape*. I warn you with regard to rape that it is one of those accusations which are very often brought against medical men, and especially against dentists who have given an anæsthetic to a single woman without a witness being present. You must be exceedingly careful about giving an anæsthetic to a female without a witness being present. Rape sometimes is merely a delusion on the part of an hysterical woman; you must beware of that. On the other hand, *rape* must often be regarded as one form of insanity. Very often rape has meant more than a mere gratification of the sexual passion, it has been a sexual act accompanied by brutality; it is one of the forms of insanity which is recognised by all asylum keepers. A few years ago London was startled and horrified by a succession of murders committed by a man who was never identified, but who was known popularly as "Jack the Ripper." A number of women were found who undoubtedly had been, one cannot say violated, because most of them were prostitutes; but they had been killed during the sexual act. And more than that, you may remember a painful case which occurred some three or four years ago in which a young man who had been in a well-known idiot asylum near London did exactly the same kind of thing in Kensington. He met a woman there, copulated with her, and killed her. When I deal with insanity I shall deal with affective insanity. There is an ideational insanity and an affective insanity. Affective insanity may be defined as the fact that a man is affected abnormally by normal conditions. Shakespeare says:

"Some men there are love not a gaping pig;
Some that are mad if they behold a cat;
And others when the bagpipe sings i' the nose
Cannot contain their urine; for affection,
Master of passion, sways it to the mood
Of what it likes or loathes."

(*Merchant of Venice*, Act IV, Sc. 1.)

That was probably written about the year 1600, a great many years before affective insanity was talked about.

You may take it that a great deal of sexual crime is a form of affective insanity. Again, you must remember that rape is most often perpetrated upon children of tender age. Caspar collected a number of cases of rape. He recorded 136 cases, and out of these no less than 99 were effected upon children of between two and a half and twelve years of age; 20 between twelve and fourteen, so that under the age of fourteen years, or before puberty, there were 119 out of the 136 cases. From fifteen to eighteen years there were 8 cases; from nineteen to twenty 5 cases; 1 of forty-seven; and 1 of sixty-eight. I want to insist that the large majority of the victims were children of tender years. Why? One reason is to be found in a very old tradition, mentioned in some of the mediæval writings, that one of the ways of curing obstinate venereal disease, notably gonorrhœa, was copulation with a virgin, and there is no doubt that a great deal of rape has been perpetrated with that intent. I warn you that these and other sexual crimes are a great deal more common than you have any idea of. You never get a country assize without cases of this kind; they are not a class of case which it is advisable to publish in detail, and as a rule in the general press there is very properly no mention of them.

The definition of rape is "the carnal knowledge of a woman forcibly and against her will." Now first of all we ask "what is force?" The law lays it down strictly that "force" may be moral force. For instance, if a man were to threaten a woman that he would bring her into trouble with her mistress or the establishment she was working in, or make it difficult for her to get her livelihood, that is force in the eyes of the law. It is very important to remember that moral force is quite as strong as physical force. The next point is "What is carnal knowledge?" There again the law has laid it down very strictly indeed, that if the vulva be touched by the penis that is carnal knowledge. The act of insertion is not necessary, the act of emission is not necessary; so that you must remember that, technically and legally, if in obedience to moral force or a threat the penis touches the vulva that is rape. I would emphasise the fact that a rape of this character leaves no physical traces behind, and between a rape which leaves no physical traces at all and that which leaves changes of a gross kind there is every gradation. Of course if

the size and development of the male and female is very different, as in the case of a big man and a little child, which has often happened, you may find injuries done to the sexual organs of the female sufficient to cause death. The vagina has been split, and peritonitis has resulted, and so forth. There was one very curious case near Edinburgh, where a couple of men riding on a cart met a young woman in the road. One forcibly held her, and the other had connection with her. Not only so, but they filled the vagina with stones and all sorts of things from the roadside, and treated her with great brutality; and that is not an isolated fact at all. We must all be ready to recognise that such cases are probably cases of affective insanity. You may say, as lawyers always say, "Oh nonsense, the man is a brute, and wants to be punished." Quite right; he is a brute, and wants to be punished; but if a man has affective insanity are you going to cure him of that insanity by two years' hard labour? When he comes out of prison and is fed again, will he not wax fat and kick just as before? It is a very nice point as to whether the pathological view of some of these crimes, or the strict legal view that they are crimes and not diseases, is the best one for the public. In considering the punishment for crime of every kind, I am very much inclined to say that in the case of young offenders and first offenders one should always make an example of them and inflict corporal punishment, which may be, and often is, a preventive, not only to the boy, but to his fellows. With juvenile offenders, if you give them a good short sharp punishment I think you will very likely do good. But when you deal with an adult and a more or less hardened sinner, the sooner we banish from our minds any idea of vengeance the better. "Vengeance is Mine, I will repay, saith the Lord." We have to get away from the idea of vengeance; all we have to do is to protect society. Take the question of rape or assaults in railway carriages, which are of the same order, and you find the press crying out for flogging. Physiologists will tell you that flogging is the worst thing you can do. But if you take the physiological view that the man is mad, you put him under lock and key and keep him confined for life. The pathological view of some of these crimes is very much safer for the public than the legal view. You may hear of a man being guilty of a tremendous crime. One man will say, "The

brute, flog him." Another will say, "Poor devil, put him in an asylum." The surest protection to society is to put him under lock and key, and not let him come out again. I do not suppose the public would tolerate another possibility in connection with sexual crimes, viz. the unsexing by surgical methods; but these are subjects upon which you ought to endeavour to make up your minds. I do not mean to say at all that punishment is not a very good therapeutic agent; I am inclined to think that the *betula alba* or common birch might with advantage be put in the pharmacopœia.

Well, to proceed. In cases of rape, when you are called to such a case, note the time. There is a law in Scotland, not in England, that accusations of rape must be brought within three days. Of course there are many cases of rape which are a little like what Byron describes —

"A little while she strove, and much repented,
And, swearing I will ne'er consent, consented."

That is not rape, and it is very important to remember that. There must be justice, and I hold very strongly that a woman may rape a man as much as a man may rape a woman. A woman may rape a boy (morally, not legally), and that also has happened not seldom. A girl, having consented to the act, recognises later that she is pregnant, and to get out of her trouble she says she was not a consenting party, and that there was force employed. In the history of rape such cases are not at all uncommon. When you are called to such a case you see what injury has been done,—whether there has been a recently ruptured hymen, whether there have been more serious injuries, and whether there are any marks or bruises about the body; and you would examine the clothing for seminal stains. In the same way you may find evidence about the person or clothing of the accused.

Now we must remember one or two things,—that sometimes after the acute specifics in children there may be not only purulent discharges from the vagina, but there may be extreme sloughing of the genital organs. In the out-patient room you will often have children brought to you with the statement by the mother that some lodger or other man has been taking liberties with the child. Now in one point of view, when cases of that kind are brought they are very serious. If it be true, and the case be taken to its logical conclusion, it means

two years' hard labour or a term of penal servitude for the man; therefore you must be very careful how you give an opinion on insufficient evidence, and remember that there is such a thing as blackmail in this world. You must make sure in such a case that the child has not been tutored. You will very often find a child in such circumstances using the expression that the man touched her with his "person." Now that is an adult expression, and when you hear such an expression you have the right to revolve in your own mind how far the child has been tutored. You must also remember that in dirty children a little leucorrhœa is not uncommon; you may even have considerable sloughing. In works on medical jurisprudence it is stated that an animalcule may be present in the vagina which somewhat resembles a spermatozoon, the *Trichomonas vagina*.

Sometimes you are asked to say whether a girl has been violated, whether or not a girl is *virgo intacta*. I would have you go very cautiously and very circumspectly to work on any such examination, and I almost think you would be right to say that you cannot tell, and that therefore you decline to give an opinion. It is, I suppose, true as an average fact that after connection the hymen is ruptured and undergoes considerable shrinkage; but it is a fact that hymens vary in quantity and quality; that while some are big and easily ruptured, others are very much more distensible. Again, you must remember that the hymen has been seen in a woman known to be a prostitute. Again, a hymen may be ruptured by other means than by carnal intercourse, and it is obvious that mere mechanical rupture of a hymen may be produced by anything which is big enough to do it. Therefore, by inspecting the hymen you are not competent to say whether that woman is *virgo intacta*. If you are called upon to examine it might not be advisable to refuse, and you would say that the hymen was or was not ruptured, but that it might have been ruptured by disease or by an accidental injury of another kind, and you could not take upon yourself to say whether the conditions were caused by sexual intercourse. I think that is the proper line to take.

There was a case some years ago which excited a great deal of attention, and one of the medical witnesses was found to declare that the girl was *virgo intacta*. That was all very well, but I think

it was saying more than he ought to have said. For instance, supposing you are asked to examine a girl, and you find the hymen has been ruptured, and there is very little hymen left. Suppose, in consequence, you allow yourself to say that she is not *virgo intacta*, you may blast her reputation for life, and quite unjustly. To what extent rape may be committed between a man and woman of about equal power is very doubtful.

Now I would remind you of the provisions of what is called "The Criminal Law Amendment Act, 1885." That law defined certain sexual crimes. For instance, it lays down that procuration of women under twenty-one is a misdemeanour punishable by two years' hard labour. It was said at the time of the passing of the Act that there were agents in this country procuring women for prostitution in other countries, and so forth. Then procuring the defilement of women by threats, fraud, or the administration of drugs is a misdemeanour punishable by two years' hard labour. The defilement of a girl under thirteen is a misdemeanour punishable by penal servitude for life. The attempt to have carnal knowledge of a girl under thirteen is a misdemeanour punishable by two years' hard labour, and offenders under sixteen years of age may be whipped. Carnal knowledge of a girl between the ages of thirteen and sixteen, or carnal knowledge of an idiot or imbecile girl, is a misdemeanour punishable by hard labour. By this Act it is a misdemeanour, punishable by hard labour, to have carnal knowledge of a woman under the age of sixteen. That is all very well; but prostitutes do not carry their birth certificates about with them. We have got to look at the protection of the male as much as at the protection of the female, and we have no right to pass laws which give women facilities for blackmailing. It is this point which has brought many of these cases to nought. There are many girls of sixteen 5 ft. 9 in. high, and strong in proportion, and it is impossible to tell whether they are sixteen, or eighteen, or twenty.

The question has arisen whether a pregnancy may arise after rape. That is an idle question, and it is no use trying to decide.

The detection of seminal stains is sometimes an important matter. It is sometimes an easy matter, and sometimes it is very difficult. If rape has taken place in the open country you may get mud, dirt, urine, fæces, discharges of blood, semen, etc.,

all mixed up together; and then, of course, it becomes a very difficult thing to detect spermatozoa. But when you find unmixed seminal fluids upon linen you may form some conclusion about them. The best way of examining them is to take a few shreds of the suspected stain, steep it in distilled water very feebly acidified, and then examine with the microscope. In the present day there are various methods of staining which may help you.

I have now to say something about *blood-stains*. A policeman may bring you a piece of linen, let us say, and, *apropos* of murder, or rape, or anything else, he may ask your opinion as to whether a certain stain is blood or not. When you are asked such a question proceed very circumspectly. I have often asked students at examination, "If a policeman were to bring a piece of garment with a spot upon it, and he asked you whether it was blood, what would you do?" The answer in many cases has been, "Oh, I would use the spectroscope." I ask, "Have you got a spectroscope?" Answer: "No, sir." "Then you would not use it." There is a tendency very often to run after the last new thing, and to run after the most out-of-the-way thing, and to forget the common things. I want to say about blood what I have said about many other things—that it is not detected by any single test. The detection of blood is by the convergence of evidence on to one point. Here is a fabric, and if that were brought to me I should record first of all the size and character of the piece of linen, and I should measure it. I should say it was somewhat irregular in shape, three inches by four inches, with a torn edge; that the linen had been stamped, and that I could see the letters—part of an I, L, and V, and parts of C, O, and L. I should examine the fibres with the microscope, and say, probably, in this case that the fabric was made of cotton; that it was mainly white, but that it was crossed by thin lines of a very faint blue. Sometimes you may get a mixed fabric. It is a very easy thing to put a few fibres under the microscope and to describe them, and a proper description of the fabric might lead to the identification of the prisoner. Having described the fabric upon which is the stain, now proceed to describe the stain. Be careful to handle it gently and not to work it about. You have to come to a knowledge of that stain in various ways. First of all I find that the stain has made the fibres of the material

distinctly stiffer. An iron-mould does not do that, and there are many other stains which do not do that; neither would an ordinary dye. But this piece of fabric is distinctly stiffened. The next thing is to examine it with a pocket-lens, and by this means you may very often succeed in seeing definite little clots upon it. I do not see any clots in this, therefore I content myself with the fact that it stiffens linen, and that fact goes a long way towards blood. When I was Examiner at the University of London, I once gave as a test for the Honours Examination a fabric stained with a mixture of wheat-flour paste and logwood. It was astonishing to find how many of those men said that the starch granules in that paste were blood-corpuscles. Therefore be careful to take all circumstances into consideration.

An ordinary blood-stain is red, but when they are very old they may lose their characteristic colour. The next point in favour of the stain being blood is that it readily gives up its colouring matter to water. If you take some water and put into it a piece of the fabric and shake it, you very soon find that the fabric parts with the colouring matter to the water. Of course the time occupied by solution depends very much on the age of the stain. If the stain is very old and dry, the solution may take some time to come about. The stain I have here is between twenty-four and forty-eight hours old. You see I have a red solution from it. I pour off some of the red solution and heat the remainder. Now please excuse me if I allude to a very common thing. Sometimes I question students on this point. They say the colouring matter is soluble in water, and I say, "Would you use hot or cold water?" Many a man hesitates, and does not give the answer. *Of course* you use cold water, because blood contains albumin; and if you coagulate your albumin—that is if you use water above 180°—you defeat your object. On boiling this solution I get it muddled and coagulated, and the coagulum is not soluble in a small quantity of nitric acid. Therefore I have something red, clotted, soluble in water, which contains albumin. If it is not blood, what is it? I pause for a reply. That really is the test for blood. But remember that you must not use one test only. There are several confirmatory tests for blood, but never omit this preliminary one, because it is the most important. One of the most common of the

confirmatory tests is tincture of guaiacum and ozonic ether, which you know well, and are using all day in the wards. But remember there are other objects which cause that blue coloration with tincture of guaiacum and ozonic ether, such as sundry vegetable juices. It is not exclusively caused by hæmoglobin. I think men are sometimes under the impression that guaiacum and ozonic ether is an infallible test for blood, but it is not; but it is a very useful confirmatory test. Another test of very great use is the microscope, because by it you can see the corpuscles. Another confirmatory test is the spectroscope.

Now remember what the spectroscope tells you. The spectrum of hæmoglobin is characteristic; it gives you two absorption bands between the green and yellow, and between lines D and E of the spectrum. Now spectroscopy is almost a science by itself, and the determination of the exact position of absorption bands is not a very easy matter; and I would warn you that there are red colouring matters which give absorption bands very close to those which are caused by hæmoglobin, and I remind you that the test of hæmoglobin does not consist so much in the occurrence of two absorption bands in a particular position, as it does in the fact that these absorption bands can be made to alter their form and position, that the two can be changed into one. And here I have before me a rough sketch of these spectra, showing that after reduction, either by carbonic acid or ammonium sulphide, the two absorption bands are united into one, and the single band is nearer the green than the yellow, a little nearer E than it is to D; and then, by shaking the blood in air you oxidise it, and then you get two.

There is another test for blood, and that is the obtaining of hæmin crystals. That, again, you have seen in your physiological lectures. Add a speck of sodium chloride and a drop of acetic acid to a drop of blood, and let the blood evaporate, and then you get hæmin crystals, which, I remind you, are rhomboidal in shape and reddish in colour. Those tests all help you to say that a given spot on a fabric is blood; but by far the most important of these is the first I showed you. Moreover it can be carried out with a little cold water and a flame. By that alone I think you would be justified in saying that in all probability the stain was blood. An important point is that blood upon a garment

very often helps to convict a criminal, and the point is, "Can you say that blood upon the clothing of a suspected criminal is human blood?" In the present state of our knowledge I should say emphatically no. A great deal of work has been done in measuring the corpuscles, and it has been shown that the corpuscles of different animals vary in diameter and size generally. But what I wish to say is that our knowledge on this point is not sufficiently precise, nor is it sufficiently easy to apply, especially in old stains, to hang a man upon. The microscopic examination of blood, however, is exceedingly important. A man was arrested on a charge of murder or wounding, and blood was found upon his garments, and this blood was put under the microscope, and it was found to contain, not round corpuscles, but oval corpuscles; it was the blood of a bird. The man was a poulterer, and the spot of blood upon his garment was not any sign of a crime, but simply a mark of his profession. So, again, you might find the nucleated corpuscles of a fish; and that would be a valuable piece of evidence, assisting not to incriminate, but to exculpate the suspected person. Now when you go to examine a stain with the microscope you may find other important evidence. For instance, you may find a hair, and you have to determine whether it is a human hair or whether it is the fur of some animal. Careful microscopical examination in this respect is sometimes of great value. You may find that the hair found upon the blood-stained garment or upon the knife is identical with the fur worn by a murdered woman. If the two hairs exactly coincide that would be a very important link in the evidence.

Now as to examining knives. A knife may be brought to you perfectly clean, and you want to see whether there is any blood upon it. Suppose a knife has been used for a murderous act and has been wiped clean. The place to look in is the nick used for opening the knife and in the name-stamp. It is there that the stuff is wiped in, and by picking it out with a pin you may get some solution containing hæmoglobin, and that would be a very valuable piece of evidence. It is important to remember where to look and what to look for. Menstrual blood is said to be acid, and the presence with blood-stain of vaginal epithelium and so forth would be an important piece of evidence.

IN publishing in the form of an atlas a series of skiagrams showing the ossification of the bones of the hand and wrist Mr. John Poland has conferred a boon upon the medical world. The atlas enables the now numerous workers in the Röntgen ray process to satisfactorily determine the important difference in regard to the evolution of ossification from what has been hitherto described, and Mr. Poland has done well in issuing with the plates a full anatomical description of the bones. Every one will join with the author in the hope that in the near future all the bones of the body may be thus portrayed. The publishers, Messrs. Smith, Elder and Co., of 15, Waterloo Place, have fulfilled their part very well, and it must be a matter of congratulation both to the author and to them to have initiated the commencement of what must prove the basis of all accurate work in connection with investigation in clinical skiagraphy. It can be safely said that Mr. Poland's teaching cannot fail to attract the attention not only of the anatomist, but also of the general practitioner nowadays needing to keep so well abreast of the latest advances in medical science.

MEDICAL GOLF TOURNAMENT, 1900. — The tournament this year will be held under medal play regulations. The date is fixed for Thursday, May 10th, and by kind permission of the Wembley Golf Club the tournament will be played on their links (station, Wembley Park, Metropolitan Railway). The Committee have drawn up the following regulations:

Competitors to be divided into two classes: Seniors, with handicaps of 12 and under; Juniors, over 12 handicap. First and second prize, and prize for best last 9 holes in each class; one round of 18 holes to be played. Entrance fee, 5s., to be sent with entry stating lowest handicap, to the Hon. Sec., Medical Golf Tournament, Windlesham, Surrey, on or before 7th May.

Medal round must be started by 3 p.m. Competitors may play over the course before playing their medal round. Members of Wembley Club play with two less than their handicap.

Foursomes will be arranged in the afternoon, "Consultants v. Practitioners." Gentlemen wishing to play should notify the Hon. Sec. with entry.

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BACTERIAL VACCINES AND ANTITOXIC SERA.

BY

WALTER C. C. PAKES, D.P.H.Camb.

DISEASES which are produced by pathogenic bacteria may be said to be of two kinds—septicæmic and toxæmic. Septicæmia may be described as a condition where the bacteria live and multiply in the blood or tissues, and may be recovered therefrom by suitable means. Toxæmia, on the other hand, is the condition where the bacteria are only found locally. Both diseases are caused, at least in part, by the action of the "toxins" or poisonous products of the bacteria. Septicæmia is well exemplified by anthrax and streptococcal septicæmia, and toxæmia by diphtheria and tetanus.

Many species of animals are susceptible to certain bacteria which have no influence upon other species. For instance, the rabbit is very susceptible to the pneumococcus, whereas the barndoor fowl is practically immune. A dose of pneumococcus which would kill a million rabbits would not cause a fowl to "turn a feather." The one is said to be very susceptible, and the other immune. Between these two conditions every stage is to be found. On the other hand, there is no such condition as absolute susceptibility or immunity. The immunity which is enjoyed by certain animals from the effects of certain bacteria is natural immunity as distinguished from acquired immunity. Speaking generally, it may be said that most diseases of micro-organic origin confer an immunity from the same disease upon those recovering from it. Second attacks of enteric fever, smallpox, and the like, are rare. Thus it may be said that patients who have once suffered from these diseases have acquired an immunity from them. This is termed an acquired active immunity, and will last a shorter or longer time according to the particular disease. The immunity acquired after smallpox or enteric fever is generally life-long, whilst that acquired after influenza and diphtheria can only be reckoned in weeks.

If, then, it were possible to induce an active immunity in a patient or animal, he or it would be less liable to the ravages of the disease than heretofore. The first serious attempt in this country to produce this was that introduced by Lady Mary Wortley Montagu from the East, where it has been practised by the priests of certain cults for hundreds of years. It had been shown that persons inoculated with the virus from a variola pustule had milder attacks than those who acquired the disease in the usual manner. This practice soon fell into abeyance, because first, the disease was found to be infectious; and secondly, those who were very susceptible had very acute attacks, and many died. Jenner then introduced vaccination as a means of inducing an active immunity from smallpox. This immunity depends upon the fact that the virus undergoes a very great modification after having been passed through a cow. The virus taken from a pustule on a cow is all but harmless to man, only causing a local lesion, but it has induced a relative immunity from the major disease. This modification of virulence towards one animal by its passage through another is not of rare occurrence. Thus the bacillus of anthrax when passed through a series of guinea-pigs becomes no longer able to produce death when inoculated into a cow, and the bacillus of swine fever when passed through rabbits loses virulence towards pigs.

There are various ways of producing an active immunity, which may be, as has been stated above, either from a septicæmic or toxæmic disease. Although the two are very intimately related, and an immunity from one form will generally be induced by the production of immunity from the other, it is not necessarily so.

Active immunity may be produced—

1. By inoculating bacteria whose virulence has been reduced.
2. By inoculating virulent bacteria in doses which are sufficient to produce a lesion, but which are too small to cause death.
3. By inoculating the dead culture.
4. By inoculating the filtered toxins.

1. The virulence of bacteria may be reduced in several ways. Thus, if a cultivation of anthrax bacilli be placed in such an environment that, so to speak, all its attention is taken up in growing, it will lose its virulence. Such an environment is obtained by growing the bacillus through several

generations at a temperature of 42° C., or by growing it in broth to which a small amount of phenol has been added; or, as we have seen, its virulence towards cattle may be reduced by passing it through guinea-pigs. When an animal is inoculated with such cultures there is a reaction, either local or constitutional, but the animal eventually recovers. After recovery it is found that a similar inoculation is followed by slighter results, and that in order to produce a similar result a more virulent bacillus must be injected. After a series of inoculations with bacilli of increasing virulence, it is found that the animal will sustain a very much larger dose of virulent bacilli than suffices to kill an animal not previously treated (called the control animal).

This method was employed by Haffkine in his anti-cholera injections. He attenuated his cholera vibrios by growing them in a current of air, and then gave a series of two or three injections of the attenuated bacteria to a person, and finally an injection of a virulent culture.

2. Since a certain number of bacteria must always be injected into an animal in order to produce death, there will always be a certain number smaller than this which will not produce death. This number will depend upon the virulence of the bacteria, and upon the susceptibility of the particular kind of animal. For instance, it was found that one cubic centimetre of a broth cultivation of the typhoid bacillus was required to kill a rabbit weighing 700 grammes; in this case half a cubic centimetre would be a sublethal dose. If then a rabbit were injected with half a cubic centimetre of the culture it would be ill, but subsequently recover. If after recovery a cubic centimetre were injected the animal would be ill, but would not die. After further injections of larger doses, 1·5 c.c., 2·0 c.c., etc., the animal would be able to sustain a dose of 10 c.c. with less disturbance than was produced by the injection of the first half cubic centimetre.

This method is employed in order to render animals immune from such organisms as the pneumococcus and the streptococcus, but it has not been employed in the case of the human subject.

3. A killed culture may, according to the medium upon which it was grown and its subsequent treatment, contain the intra-cellular or extra-

cellular poisons, or both. If a culture of the tetanus bacillus be washed free of all its extra-cellular poisons, and injected into a susceptible animal, such as the guinea-pig, the latter will not die, whereas if $\frac{1}{100}$ c.c. of the extra-cellular poison be injected it will probably succumb. On the other hand, a rabbit will not succumb to such a small dose of the filtered extra-cellular poison of the typhoid bacillus as of the dead bodies. It may be said, therefore, that the extra-cellular poisons of some bacteria are more pathogenic than their intra-cellular poisons, whereas with others the converse is true. This method of producing immunity has been used in many cases, and in the human subject notably in Wright's typhoid vaccine. He takes a virulent culture of the typhoid bacillus and grows it in broth for a certain length of time, and then kills the culture by exposing it to a temperature of 65° C. This killed culture constitutes the vaccine, which is next standardised by finding the minimal fatal dose for guinea-pigs. The dose for man is then arbitrarily fixed. The injection of this vaccine into the human subject is followed by both local and constitutional disturbance; the local consists of swelling and redness at the seat of inoculation, and the constitutional of headache and slight collapse. After the reaction is over the patient has now acquired an active immunity, which may be increased by subsequent injections.

4. Some bacteria produce toxins or extra-cellular poisons which are extremely poisonous; among these are diphtheria and tetanus bacilli, and animals are immunised by these filtered toxins. The process of immunising a horse against diphtheria is typical, and is somewhat as follows:

A virulent diphtheria bacillus is obtained, and grown in alkaline broth for about a fortnight. After this the culture is filtered through a porcelain bougie, and the resulting clear fluid constitutes the toxin. A small dose of this—say a cubic centimetre—is injected into the subcutaneous tissue of the neck. The result of this injection is the formation of a localised swelling and a rise of temperature. When this has quite subsided a further injection is given, and then subsequent ones, care being taken that no injection is given until the previous swelling has subsided. When a partial immunity has been obtained the toxin is injected intra-venously in increasing doses; and finally, when the horse has acquired a considerable degree

of immunity, it is inoculated with living virulent cultures. The whole process takes several months, but at the end of the time the horse is unaffected by doses of virulent living cultures many times greater than the fatal dose, and the horse is said to have a high immunity.

It will be seen that this active immunity is not acquired in a moment, but requires a considerable time. It is of no use, therefore, as a curative, but only as a prophylactic agent. When it is once acquired it lasts for a very considerable time. When it is acquired the body of the animal has undergone certain changes, and reacts differently towards the organism than it would have done had it not been immunised.

Pfeiffer showed that if a guinea-pig which had been immunised against cholera were injected intra-peritoneally with a culture of cholera the organisms rapidly changed in appearance. Instead of being rod-like and actively motile, they became globular and lost their motility, and were finally eaten up by the phagocytes. He next found that if some of the blood-serum of an immune guinea-pig were mixed with the culture and injected into the peritoneal cavity of a normal guinea-pig the same phenomenon took place. Bordet subsequently observed that this phenomenon also took place outside the body. With regard to the toxins, they appear to lose their pathogenic power. It may be said, then, that there is something in the blood, and, of course, other tissues, which neutralises the toxins, and so renders them harmless, and which prevents the growth of the bacteria in some way or other. The serum is not necessarily bactericidal, since the organisms may be able to grow in the serum outside the body.

A very startling effect is seen in addition to the above, as was pointed out by Gruber and Durham. If a culture of cholera be mixed *in vitro* with some serum from an immune guinea-pig the vibrios lose their motility and become agglutinated. This agglutination may be seen both in the test-tube and under the microscope, and consists in the bacteria herding together in large clumps. Whenever an animal has been immunised against, say, typhoid, the serum invariably reacts in this manner—sometimes more markedly, sometimes less, according to the degree of immunity. This fact was made use of by Widal, and is the foundation of his reaction.

When an animal has thus acquired an active

immunity from an organism it contains these antagonistic bodies, "agglutinins," and it is found that its immunity can be transferred to other animals by injecting them with its blood-serum. This immunity, which is the result of the injection of the serum, is called passive immunity, and the serum is spoken of as the antitoxin. This immunity is comparatively transient, but it is produced almost at once. The serum or antitoxin may be used as a curative, since it contains the agglutinins which neutralise the toxins and prevent the growth of the bacteria. This is the immunity which is produced when antidiphtheritic or antitetanic sera are injected. Some sera are antitoxic and antimicrobial, others are only antitoxic or antimicrobial, but the action is of a similar nature; that is, something is added to the blood of the susceptible animal which prevents the further progress of the disease, if it has not already gone too far and the toxins have actually damaged the cells of the body.

Iridochoroiditis due to Intestinal Auto-infection.—Elia Baquis reports a case in which the eye symptoms were clearly referable to prolonged attacks of enteralgia and obstinate constipation. That there is a close connection between intestinal lesions and diseases of the eye is well known. Given an infected area in any part of the organism, distant portions may be affected by living germs or their toxic products. In the case reported the author believes that the process was a chemical one. Toxic products of digestion, not having been eliminated, were absorbed into the circulation and injuriously affected the eyes.—*Medical Record; Medical Bulletin*, April, 1900.

The Specific Serum.—Gino Galeotti says that Ehrlich's theory is that in order that any substance should be toxic to the organism it must possess the power to fix itself chemically upon the molecules which constitute the living protoplasm of the organism itself. This theory the author finds in complete harmony with the results of experimentation in regard to the action of serums and antitoxins.—*Lo Sperimentale*, No. 1, 1900: *Medical Record*).

TREATMENT WITH ANTI-STREPTOCOCCIC SERUM.*

BY

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MR. PRESIDENT AND GENTLEMEN,—Before discussing the clinical evidence of the value of anti-streptococcic serum, I will draw your attention to certain points in connection with streptococcal infection. There are a number of pathological processes presenting different clinical symptoms which are caused by the invasion of the body with streptococci. We shall have to consider whether these processes are manifestations of a single disease, or whether they are distinct pathological entities. The processes in which streptococci are found are: (1) all cases of cutaneous erysipelas; (2) most cases of puerperal fever; (3) many cases of septic trouble passing under the names of septicæmia, phlegmonous erysipelas, pyæmia, infective lymphangitis, infective endocarditis, etc.

In determining whether a given disease is specific, we can approach the question from two points of view;—we can consider the matter from the clinical or from the ætiological side. If we were to rely entirely upon the clinical symptoms exhibited, we should decide that many of these processes represented distinct diseases. A case of cutaneous erysipelas, for instance, appears at first sight to be in no way connected with a case of puerperal fever, so different are the clinical symptoms. But different symptoms are exhibited by a localisation of the same virus in different tissues. The symptoms of tuberculous meningitis are different from those of tuberculous arthritis, yet they are both caused by the tubercle bacillus. A simple inorganic poison like lead may give rise to cerebral symptoms, to paralysis, or to colic, according to its localisation in the tissues.

The severity of the symptoms is of no importance in determining whether two diseases are

* Delivered before the Harveian Society.

identical or not. It is well known that two patients may contract a specific disease, such as scarlet fever, from the same source, the one being severely and the other slightly affected.

The character of the lesion depends to a great extent upon the strength of the virus in relation to the resistance of the patient. In faucial diphtheria, for example, all gradations of inflammation from a simple reddening of the fauces up to the formation of thick membrane are met with. There is thus no difficulty in considering the serous exudation in a case of septicæmia to be caused in the same way as the pus in a case of pyæmia.

The character of the lesions depends also upon the localisation of the virus. The same virus will produce different lesions according as it is located in the lymphatic system, the circulatory system, or other parts. There is clinical evidence that cases of typical cutaneous erysipelas may gradually merge into septicæmia or pyæmia.

Another important factor in determining the specificity of an infective disease is the source of the infection. A specific disease is said to "breed true"; for example, a case of scarlet fever gives rise to other cases of scarlet fever, and not to other diseases. It is by such means alone that we are able to distinguish between many cases of German measles and ordinary measles, for their symptoms are in many cases exceedingly alike.

Now, as a rule, a case of erysipelas is derived from another case of erysipelas, and a case of puerperal fever from another case of puerperal fever; so that, at first sight, we might look upon erysipelas and puerperal fever as separate and distinct diseases. But when we consider the matter more carefully, we find that a case of erysipelas may give rise to puerperal fever in a pregnant woman, and that any one of the conditions associated with streptococci may give rise to any other—indeed, they are mutually interchangeable. Why one type is more likely to give rise to the same type than to another type is easily explicable by a consideration of the mode of conveyance of the disease.

A case of puerperal fever is more likely to give rise to puerperal fever than to erysipelas, because it is conveyed by instruments or by the hands of the midwife to the uterus.

The above considerations give us much suggestive evidence in favour of the view that the conditions associated with streptococci are manifestations of a single disease. The absolute proof must depend on bacteriological evidence of the identity or non-identity of the associated streptococci.

In the early days of bacteriology this was considered to be quite an easy matter, but with further knowledge we find it an exceedingly difficult matter to identify a micro-organism, and to distinguish between it and closely allied species. In determining the species of a bacterium we cannot rely upon any single characteristic, such as morphology or cultural appearances. The whole of the characters must be taken into account, and the relative importance assigned to each. The characters upon which we most rely are morphology, mode of growth, pathogenic properties, and the reaction towards the serum of immunised animals. Most of these characters are capable of very considerable modification. A bacterium which under a certain condition has the shape of a coccus, may under other conditions have the shape of a bacillus; and it is easy to modify the mode of growth by long-continued cultivation on various media. Virulence, upon which the pathogenic effects depend, is the most variable of all the characteristics. It is easy to render a highly virulent bacterium non-pathogenic, and by appropriate means it is possible to convert an almost harmless bacterium into one of intense virulence. The pathological lesions produced are by no means specific; they depend to a very large extent upon virulence. A highly virulent bacterium which produces a rapidly fatal septicæmia will, when attenuated, only give rise to a local abscess. From such considerations as these, you will understand that it is often difficult to determine whether two bacteria belong to the same or to a different species. The bacteriological diagnosis is in some cases almost as difficult as the clinical. These difficulties are especially met with in deciding whether the streptococci found in the various diseases above mentioned are identical or not.

Formerly the streptococcus found in erysipelas was considered to be a distinct species from that found in septic conditions, the former being designated the streptococcus erysipelatos, and the

latter the streptococcus pyogenes. This view accorded with the prevalent ideas as to the specific character of erysipelas. The chief distinction drawn between the streptococcus erysipelatos and the streptococcus pyogenes was the different pathogenic effects produced by inoculating animals. But further experience has shown that these differences are not to be relied upon, and it has been demonstrated that the same streptococcus may give rise either to erysipelas or to some septic conditions. Nevertheless, if streptococci are obtained from various sources it is found that they differ from one another in certain points. Whether these differences are sufficient to constitute distinct species is still questionable, but they are of great importance in connection with serum therapeutics. To this subject we shall shortly recur; in the meanwhile I would urge that the character of the lesion in the patient gives us no indication of the kind of streptococcus present. On the one hand, a streptococcus obtained from a case of erysipelas may be identical with one obtained from some other condition; while on the other hand streptococci obtained from apparently similar lesions may differ from one another in a manner which we shall presently indicate.

A streptococcus, even when isolated from a severe case of disease in the human subject, is not at first very virulent to laboratory animals. The virulence can, however, be raised by what is called repeated passages through rabbits. By this term we mean that a rabbit is inoculated, and after death a fresh cultivation is made from the blood. With this cultivation another rabbit is inoculated, and so on through a series of animals. After a number of passages, a minute quantity of a cultivation introduced into the peritoneal cavity of a rabbit rapidly causes death. A post-mortem examination shows the usual appearances of septicæmia, and on microscopical examination the blood and tissues are found to be crowded with cocci. The cultivations used by Marmorek were so virulent that one hundred thousandth part of a cubic centimetre of a fluid cultivation was invariably fatal to rabbits. The virulence of the cultivations can be maintained by cultivation upon a medium composed of broth with blood serum or ascitic fluid. Rabbits can be immunised by previous inoculation with

attenuated cultivations; after recovery they are found to be unaffected by inoculation with virulent cultivations.

In 1892 Roger showed that the blood serum of immunised rabbits possessed the property of protecting other rabbits against infection with the streptococcus if it were injected immediately after inoculation. These experiments were confirmed by Mironoff, and a large amount of work upon this subject has since been done by various observers.

The results of these various observers were frequently discordant, some succeeding and some failing to obtain a protective serum. Not infrequently one observer succeeded with his own serum, but failed with that of others in eliciting protective properties. But the outcome of these researches quite definitely showed that it was possible to obtain a protective serum. The discrepancies above alluded to were due partly to faulty methods of testing, partly to the feeble protective power of the serum, and partly to the fact that it loses its potency after being kept. Still more important is the fact that a serum may protect against one streptococcus, but not against another which is in other respects identical. Van de Velde took two streptococci which he called streptococcus A and P; and with each of these streptococci he immunised a horse. The serum from horse P protected against the streptococcus P, but not against the streptococcus A; while the serum of horse A protected against streptococcus A, and to a slight extent against streptococcus P. By immunising a horse both with streptococcus A and with streptococcus P, he obtained a serum which protected against both. Streptococci, apparently identical, can thus be distinguished from one another by means of the serum of immunised animals. In order to treat patients, it was necessary to obtain large quantities of serum. This was done in 1895 by Marmorek, Charrin, and Roger, and by Denys and Leclef, from horses, mules, and donkeys. Bulloch, in 1896, was the first to describe the preparation of serum in this country. At the present time serum is supplied by a number of different institutions, both in this country and abroad.

The serum is now prepared either from the horse or the donkey; the former is usually

chosen as it is less affected by the necessary inoculations. For the inoculations, virulent cultivations are used, these cultivations being obtained by repeated passages through rabbits. The inoculations are made into the subcutaneous tissue of the shoulder. At first a small dose consisting of about one cubic centimetre is employed. The injections give rise to considerable swelling, attended by pyrexia and malaise. Individual horses vary enormously in susceptibility; some horses are severely affected by a small dose, and others are hardly affected at all by a large dose. After recovery from the first inoculation, a second inoculation with a larger dose is made, and the process is continued for several months, ultimately as much as 100 cubic centimetres being used for a single dose. The horse is then bled, and the serum allowed to separate; it is subsequently filtered through a Berkefeld filter, and, after the addition of a small quantity of an antiseptic, it is transferred to small bottles which are plugged with india-rubber stoppers. The whole operation is performed with strict aseptic precautions. The horse must not be bled too soon after the last injection, as the serum was found by Marmorek to be toxic to rabbits when removed as late as fifteen days after inoculation.

Van de Velde recommends using several strains of streptococci for inoculating the horses, so as to obtain what he calls a "polyvalent" serum.

The serum is standardised in the following way. The minimal fatal dose of the streptococcus is first ascertained by inoculation into the peritoneal cavity in a series of rabbits, with measured doses of cultivations in media consisting of broth and ascitic fluid or human blood serum. Measured quantities of the serum are injected into the peritoneal cavity in another series of rabbits, and twelve hours subsequently they are inoculated with tenfold fatal doses of cultivations. The smallest quantity of serum necessary to protect is an indication of its strength. Marmorek's serum was of such strength that 0.2 c.c. when thus tested was protective. Unfortunately the strength of the serum supplied by various institutions is not always stated, and in all probability many samples are exceedingly weak.

We do not know how long the serum preserves its potency; it appears to deteriorate rather quickly.

Marmorek found that, with a serum of which 0.2 c.c. protects when injected twelve hours previous to inoculation, 1 c.c. was required to protect three hours after infection, and 5 c.c. if injected two hours later.

The antistreptococcic serum is anti-bacterial, but it is probably not antitoxic, that is to say, it protects against living cocci, but not against their toxins. Van de Velde has shown that the serum acts in two ways: it retards the growth of the streptococci, and it acts upon them in such a way as to render them capable of being englobed by phagocytes.

There are considerable difficulties in the way of estimating the value of treatment with antistreptococcic serum. Statistics derived from published cases are valueless, because the unsuccessful cases are not necessarily recorded. Besides, the diverse type of the conditions submitted to treatment renders statistical evidence inadmissible. We must therefore fall back upon the opinion formed by individual observers of the effect of the serum upon the course of the cases under their care.

In the various journals many cases treated with serum, both with satisfactory and unsatisfactory results, have been recorded. We must exercise great caution in attributing the cure of any disease to the use of a particular remedy. We all know how frequently cases are brought forward in support of the value of any new remedy, which further experience shows to be useless. Because a patient recovers under a particular line of treatment, it does not necessarily follow that the result is due to this treatment. While fully alive to such errors, I am nevertheless satisfied, from the cases published in the journals and from my own experience, that antistreptococcic serum is a most valuable remedy in certain cases; on the other hand, in cases apparently identical it exerts no influence whatever. This is capable of explanation when we remember that an antistreptococcic serum may be found to exert a protective influence over one streptococcus, but to exert none whatever over a streptococcus which in other respects is identical.

I will quote a few examples of the different types of streptococcal infection in which the serum has proved to have been of extreme value.

These cases I have obtained either from the journals or from my own experience.

PYÆMIA.

Mr. Lawford Knaggs has recorded* the case of a man, aged forty-three, who was admitted on February 9th, 1898, into the Leeds General Infirmary, suffering from a large perineal abscess communicating with the urethra. On March 29th, after the passage of a bougie, he was seized with a rigor, the temperature reaching 103.6° . On March 30th there was œdema of the scrotum, and he had another rigor with a temperature of 104° . On March 31st there was an escape of pus from the perineal wound, after which the scrotal condition quickly subsided. He again had a rigor with temperature 103.6° . On April 1st another rigor, temperature 106° . An abscess was observed over the left olecranon. On April 2nd the abscess was opened. April 3rd the temperature was 105.4° . On April 5th ten cubic centimetres of serum were injected; this was repeated on April 6th, 9th, 16th, 17th, and 18th. Both on April 5th and on April 6th there was hæmoptysis. On April 11th an abscess which had formed on the outer condyle of the right humerus was opened. From this time rapid improvement ensued. An inflammatory patch in the subcutaneous tissue over the patella, which was first noticed on April 6th, subsided without suppuration. The temperature came down after the first injection, and after April 19th it remained normal.

SEPTICÆMIA.

Messrs. Ballance and Abbott† have recorded the case of a doctor, aged thirty, who punctured his thumb at 1.45 p.m. on June 8th, while performing a post-mortem examination upon a case of suppurative peritonitis. At 7 p.m. he suffered from pain in the thumb; red lines were observed running up to the axilla, and the axillary glands were enlarged. At 4 a.m. on June 9th the thumb was incised. At 7.30 a.m. the temperature was 103° ; at 9.30 a.m. a brilliant scarlet septic erythema was observed over the body. He gradually got worse. On June 10th the temperature was 104.7° , pulse 150 and running.

* 'Lancet,' September 27th, 1898.

† 'Brit. Med. Journ.,' July 4th, 1896.

The rash was very brilliant and in places hæmorrhagic. He suffered from epistaxis, vomiting, and coughing of blood-stained mucus. There was slight albuminuria. Three and a half cubic centimetres of antistreptococcic serum were ordered to be given every four hours. Six hours later an improvement in the symptoms was observed. On June 11th the temperature was 104° ; there were more marked subcutaneous hæmorrhages. In the night the temperature fell, but the thumb was very painful. On June 12th the thumb was incised but no pus found. The serum was increased to seven cubic centimetres every four hours; from this time rapid improvement and ultimately complete recovery ensued.

SEPTICÆMIA WITH CEREBRAL SYMPTOMS.

S. R—, aged thirteen, was admitted into Guy's Hospital on March 31st, 1896, under my care. On the previous morning a left upper molar tooth was removed, a quantity of pus flowing from the socket. He walked home, and on the way fell ill. In the evening he began to ramble, and soon became unconscious, with incontinence of urine and fæces.

On admission, pulse 156, temperature 104.5° , respiration 32. He was practically unconscious, and there was constant twitching of the muscles in the neck and face, and at times in the legs and arms. There was incontinence of urine.

The face was much swollen on the left side, the swelling being œdematous, and of a dusky red colour. The hard palate of the same side was swollen. Pus and blood oozed from the socket of the tooth removed. The lymphatic glands on the left side of the neck were much enlarged.

April 1st.—Internal strabismus. At 12 noon, when temperature was 102.6° , 30 c.c. antistreptococcic serum was given. At 10 p.m. temperature had fallen to 100° .

April 2nd.—Better. Temperature at 2 a.m. normal, but had risen at 10 a.m. to 102.2° , 30 c.c. serum administered. At 6 p.m., temperature had fallen to normal. Incontinence of urine ceased. Pus from tooth examined, and found to contain streptococci.

April 3rd.—Much improved. Temperature kept at 100° all day. Twitching ceased, 30 c.c. serum given at 4 a.m. and again at 11.30 p.m.

From this time patient continued to improve, and no further bad symptoms were observed.

ERYSIPELAS.

E. B—, a child, was admitted into Guy's Hospital on July 14th, 1899, under my care for erysipelas of the leg. The attack commenced on the previous day, the inflammation starting around an abrasion below the left knee. On admission there was typical cutaneous erysipelas, with a well-defined edge over the lower third of the thigh. The temperature was 104° , pulse 130. The child was decidedly ill. On July 15th the erysipelas had spread, temperature 101.8° , pulse 130. July 16th, the erysipelas had spread as far as the foot, temperature 103.2° , pulse 130. The child was much worse. At 4 p.m., when the temperature was 103.2° , twenty cubic centimetres of antistreptococcic serum were injected. At 6 p.m. the temperature was 100° , and at 10 p.m. it was 99.4° . On July 17th the erysipelas had ceased to spread and the child was much better. At 10 a.m. the temperature was 98.8° , but this had risen at 2 p.m. to 99.4° , when twelve cubic centimetres of serum were injected. After this the temperature remained normal, and the erysipelas quickly subsided.

PUERPERAL FEVER.

Mr. Nolan Daly* has recorded the case of a woman who had been delivered by a midwife on December 6th, 1897. He found the placenta adherent and was obliged to remove it. On December 7th the patient was doing well, but on December 8th she was seized with a rigor, temperature 104.5° , the pulse 150 and running. The lochia were absent. The uterus was washed out and quinine administered. On December 10th no improvement had occurred, temperature 103° , pulse 136; there were frequent rigors and marked tympanites. At 12 a.m. ten cubic centimetres of antistreptococcic serum were injected. At 2.30 p.m. temperature 102.1° , pulse 136. On December 11th she was much better, temperature 99.8° , pulse 108; the tympanites had disappeared. On December 12th temperature 100.8° , pulse 108, ten cubic centimetres of serum given. On December 13th temperature

99° , pulse 102. December 14th temperature 101.2° , pulse 102, ten c.c. serum given. From this time recovery was uninterrupted.

INFECTIVE ENDOCARDITIS.

I have recorded the case of a female,* æt. twenty, who was admitted into Guy's Hospital under my care on January 27th, 1897, for pyrexia and general malaise.

For seven weeks after admission she suffered from rigors and continuous but irregular pyrexia, which did not yield to remedies.

No lesions could be found until March 16th, when a pulmonary diastolic bruit developed. A diagnosis of infective endocarditis was made, and antistreptococcic serum was administered on March 24th. The injections were continued almost daily for nine weeks, 1,030 c.c. being injected in 59 days.

Up to the time of the first injection she was gradually losing ground, but she then began to improve. The temperature became normal at the end of a fortnight and remained so, except on one occasion when the injection was not given. She recovered completely, with the exception of the damaged valve. I have seen her frequently since. She has had no further pyrexia, but often suffers from cardiac symptoms.

The above instances give us an indication of the kind of case likely to be benefited by antistreptococcic serum. Generally speaking all cases of streptococcic infection are admissible for treatment. When possible a bacteriological examination should be made in order to determine whether the case is one of streptococcic infection or not. Such an examination can often be made in a few minutes by a microscopical examination of pus or exudation. In a case I saw a few months back I made a microscopical examination of the serous exudation in the subcutaneous tissue while the patient was under an anæsthetic for an exploratory incision. Streptococci were readily found, and the serum was injected while the wound was being dressed and before the patient had recovered from the anæsthetic. In some cases of streptococcic infection it is impossible to demonstrate the presence of streptococci during life; I refer to such cases

* 'Lancet,' January 29th, 1898.

* 'Lancet,' September 18th, 1897.

as those of streptococcic endocarditis, where streptococci may be absent from the circulating blood or present in numbers too small to be recognised. In such cases, and in cases where the lesions are inaccessible, we should not wait for the absolute proof afforded by bacteriological evidence. I have already stated that all cases of erysipelas, most cases of puerperal fever, and many cases of pyæmia, septicæmia, and infective endocarditis are streptococcic infections. The septicæmia of scarlet fever is almost always a streptococcic infection, but in my own experience antistreptococcic serum has not been successful in such cases. Of nine cases treated in only one did the treatment appear to have good effects, and even here the effects were doubtful, while seven of the cases ended fatally. Perhaps this is due to the scarlatinal streptococcus being a distinct variety, which is not influenced by the serum at present in the market. Low (March 19th, 1898) has, however, recorded a successful case of scarlatinal septicæmia treated by serum.

Antistreptococcic serum may be given as a prophylactic measure in certain cases. I have given it, for example, to a medical man who had cut his finger in performing a post-mortem examination on a case of septicæmia.

The serum is administered in exactly the same way as the diphtheria serum, careful aseptic precautions being observed. The dose is ten to thirty cubic centimetres, which may be given two or three times a day in acute cases, and once a day in chronic cases. If possible, it should be injected near the site of infection, for it has been experimentally shown to be more efficacious when administered in this way. The treatment should be commenced as early as possible; nevertheless in many of the successful cases the treatment was begun late. If the serum appears to have no effect, it is well to try a fresh sample obtained from another source. Mitchell Bruce ('British Medical Journal,' July 8th, 1899, page 76) has recorded a case of septicæmia where the serum from one source had no effect, but which yielded to serum obtained from another source. A polyvalent serum, as suggested by Van de Velde, may prove to be more universally useful than that at present supplied.

The therapeutic effects observed in successful

cases are a lowering of temperature, a subsidence of inflammation, and a general improvement in the patient's condition. After effects similar to those following the use of diphtheria serum may occur. These effects are due to some constituent of the serum other than the protective substance, for they may follow injection with normal serum. They comprise rashes, pyrexia, joint pains, and, in rare cases, collapse. I would draw especial attention to the fact that purpura has been described as a rare sequence to injection with serum; in some instances the purpura is certainly not due to the serum. In the case already alluded to by Ballance and Abbott, a purpuric eruption preceded the injection of serum, and it is well known that septicæmia is sometimes accompanied by hæmorrhage into both the skin and mucous membranes.

Why Serum Treatment fails in Puerperal Septicæmia.

— Since the introduction of serum by Marmorek in France simultaneously with Charrin and Rogers, it has in turn been highly lauded and condemned—chiefly condemned. The streptococcus is an extremely variable organism, and there are very marked differences between individual streptococci, which are manifested by their varying virulence, their morphology, behaviour on culture media, and toward colouring matters. I would call attention to their varying virulence. Observers have shown that serum which is produced from one variety of streptococcus may only be efficacious against infection due to that particular organism, but inert against infection produced by streptococci obtained from other sources. Against this must be considered, however, the generally accepted opinion that streptococci are all of one family, and that the serum is alike efficacious against all.

Failures in the action of serum may be attributed to—

1. Old serums.
2. Strictly antistreptococcic serum can only be efficient against toxins produced by streptococci; it must be useless when we have a mixed infection to deal with.
3. Delay in inaugurating treatment with the serum, and administering it in insufficient quantities.
4. Over-stimulation of the patient.

HERYNG, *N. Y. Med. Journ.*, April 7th, 1900.

THE SERUM DIAGNOSIS OF TYPHOID FEVER.

A Lecture with Practical Demonstration given in the
Pathological Department, University College, London,

BY

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Assistant to the Professor of Pathology,
University College.

GENTLEMEN,—As most of you are aware, the bacillus of typhoid fever is a very actively motile organism. If we examine microscopically a drop of broth in which the typhoid bacillus has been grown, we see the bacilli moving rapidly about and darting hither and thither across the field of vision. The bacilli do not come together in clumps, and if a few do come in contact and adhere, they quickly separate and resume their rapid career. If we take measures to prevent the drop drying up, *e.g.* by painting a layer of vaseline round the coverslip, the bacilli will continue in active movement for hours. If now to such a broth culture we add a little serum from a person *not* suffering from typhoid fever, having first diluted that serum sufficiently, and then examine it microscopically, the bacilli continue to move about actively as before, and show no signs of collecting together in groups.

If, on the other hand, we add to the broth culture some serum diluted to the same extent, but from a person ill with enteric fever, we find that two changes take place. One is a cessation of movement of the bacilli, and the other is that the bacilli begin to come together, and, coming together, adhere to one another and form clumps, which may vary considerably in size.

It is these two phenomena which constitute a positive serum reaction, *i.e.* the *clumping* and the *cessation of movement*, and the performance of this test consists in mixing in proper proportion the suspected serum and an active broth culture of typhoid bacilli, and ascertaining whether these two changes take place.

Theoretical considerations with regard to the nature and cause of these two changes would be out of place here. I should like to say, however, that the reaction does not depend upon the

presence of typhoid bacilli in the blood or serum used. It is only very exceptionally that we can find typhoid bacilli in the blood. It is necessary to state this, for I gather from the many questions asked me, that it is a widely-spread idea that the essential cause of the reaction is the presence of typhoid bacilli in the blood. The typhoid bacilli causing the enteric fever produce in the spleen and elsewhere toxins, which toxins by their action on the tissues, particularly on the blood, cause certain changes, apparently chemical in nature, which give to the blood and certain other fluids this property of causing clumping and cessation of movement when mixed with typhoid bacilli.

Before we can accept this test as of much value, it is obvious that at least two questions must be answered: first, does the blood of *all* cases of enteric fever yield these characteristic changes? and secondly, does the blood from cases other than typhoid fever ever give this reaction with typhoid broth?

We will consider these two questions more in detail, and I will take the latter first.

To put it in another way. Can we, whenever we get a positive reaction, unhesitatingly say that the case is one of enteric fever? We cannot say that this is always the case. However, by the careful performance of the test and with adequate controls, we can eliminate the majority of the cases in which a positive reaction has been obtained with blood taken from a person *not* suffering from typhoid fever.

Let us consider some of the causes of positive reaction in non-typhoid cases.

(a) It is a well-known fact that broth cultures of bacillus typhosus will, and not infrequently in my experience, clump spontaneously. If you examine a drop of such a culture microscopically, you find many of the bacilli in clumps, usually loose clumps it is true, but still definite and well-marked. There are, besides, many bacilli moving between the clumps, but usually not very actively motile, and the individual bacilli can frequently be seen to be much longer than usual. You will see under one of the microscopes a drop of such a broth culture, showing the long sluggish bacilli and the well-marked pseudo-clumps. Obviously if serum be added to such a broth, whether serum from a typhoid patient or not, it will show well-marked clumps, and the serum may be pronounced

to be one from a typhoid patient. Fallacy from this source is obviated at once, by either making a control experiment with normal serum, or by simply examining microscopically a drop of the broth before it is used for the test. The cause of this spontaneous clumping is not always obvious. It probably depends mainly upon the media and the methods of growth, thus cultures grown too long at blood temperature or on too alkaline a medium may show it.

(b) It has been proved that the peculiar properties of typhoid serum, *i.e.* the ability to clump and cause cessation of movement of typhoid bacilli, may persist not only for months, but even for years. Individuals may be attacked, perhaps years after the attack of enteric fever, by influenza, pneumonia, or other diseases, which may temporarily be confused with typhoid fever, and so cause their blood to be examined. A positive reaction is obtained, and yet the case turns out not to be typhoid. It is necessary, therefore, to inquire carefully into the previous history of the patient.

In this category there are a few cases which may be very confusing. I refer to those cases where the patient may have had typhoid previously, but not diagnosed as such, or the attack was so slight that there was scarcely any illness (for in slight attacks the reaction, and possibly the persistence of the reaction, may be just as marked as in the more severe cases).

Error in such cases is difficult to avoid, but I should imagine these cases where confusion could arise must be very infrequent.

(c) Normal serum, if diluted only slightly, say one in ten, will undoubtedly in certain cases cause some clumping and cessation of movement. If the serum be further diluted and again examined with typhoid broth, these changes do not take place.

I think I am well within the truth in saying that if the serum be *sufficiently* diluted, clumping with normal blood never takes place. What we are to accept as the exact numerical expression of "sufficiently diluted," however, is one to which different observers have attached very different meanings. As far as is at present known, we may say that a dilution of one in thirty, almost certainly one in fifty and higher dilutions, will prevent fallacies from this source.

When we come to consider the sera of diseases other than typhoid fever (and not normal serum as above), we find positive reactions described in many diseases, but as Cabot points out, scarcely any two observers find these pseudo-reactions in the same diseases, and in all probability, with the above precautions as to technique, controls, and sufficient dilution, they would never take place.

Summing up, therefore, this question as to reaction in cases other than typhoid, we may fairly assume that, performed with these precautions, this reaction does not take place in cases other than typhoid, or in cases which have had typhoid fever.

To consider our first question—Does the blood of *all* cases of enteric fever yield these characteristic changes? Here again we must confess—not always. For (a) it may be too early in the course of the disease. The reaction is generally obtained about the end of the first week. (b) There are many cases described in which the reaction has been known to intermit; for example, present one day, absent the next, and again present the next day or in a few days. These cases, probably from their infrequency, do not seem to be a common cause of error. (c) Undoubtedly also a few cases of proved enteric fever yield no serum reaction throughout the disease. Many undoubted cases are recorded.

The first two causes of absence of reaction obviously teach us that in negative cases we should repeat our examination of the blood at a subsequent date. The third cause is fortunately a very rare one. We may say, therefore, that a negative reaction, though not so valuable as a positive one, is yet of great value.

Summing up the whole reaction, we may say that, efficiently performed, the test is of extreme value; probably, according to most authorities, accurate in ninety to ninety-five per cent. of cases. It is, however, vitally important that the test should be done properly, and not in the careless and fallacious fashion which too often prevails.

Leaving these considerations, we will proceed to study the best way or ways of carrying out this test.

First, in regard to collection of the blood. It is usually taken from a finger or lobe of ear. Wash with water, but it is not necessary to sterilise the skin. Use a bayonet-pointed needle rather

than a round needle. If blood is obtained from the finger, it is convenient to tie a piece of tape round it first. There are two chief methods of collection: either the blood is collected and allowed to dry on a slide, or it is obtained in fine glass pipettes. In the former method the finger is pricked, and a clean slide is made to touch the drop in several places—it is best not to exceed three. The drops are allowed to dry, and the test is subsequently performed, using the dried blood. When pipettes are used, perhaps the most convenient shape is with a small bulb in the centre, and drawn out into a long, capillary process at each end. The finger is pricked and the blood is sucked, or better, allowed to flow by capillary action into the pipette. There should be a continuous column of blood, with no intervening air bubbles. It is quite easy to do this. The ends are then sealed in the flame, sealing the end nearest the blood first. The serum is allowed to separate. In this method of collection the serum free, or almost free, from corpuscles is used. When dry blood is used, a little water or inert fluid is added to the drop, and an emulsion is made, and a loop of this is used. Here the blood itself is used, and many corpuscles are taken up in the loop. With regard to the respective merits of these two methods of collection, I think undoubtedly the use of the serum from capillary tubes is to be preferred, and that the use of dried blood should be limited to occasions when collection pipettes are not obtainable.

In the hands of many observers, notably Johnston, the dried blood method has given extremely valuable results, but I think it adds somewhat to the difficulties of performing the test properly.

Secondly, what kind of culture of the bacillus typhosus should we use? This is a very important factor in the test. I have incidentally mentioned that sometimes typhoid bacilli cultures will clump spontaneously. Such cultures should obviously not be used, or not until by various means they have been deprived of this habit. Again, some cultures of bacillus typhosus will clump better than other cultures obtained from different sources, and this not only for particular cases but as a fairly general rule. Thus to mention one or two examples. In a case of enteric fever in Ward III in the hospital, the blood was examined on 3rd November in one per cent. dilu-

tion of the serum with typhoid bacilli from four different sources. A fairly well-marked reaction was obtained with one variety, a very feeble reaction with a second, and no reaction at all was obtained with the other two. In another undoubted case in Ward XI the blood examined on 6th November showed with two varieties of typhoid bacilli a marked reaction, with another variety a weak reaction, and no reaction at all with a fourth variety; these also in one per cent. dilution. Quite similar results have been obtained by Lorrain Smith and Tennant ('British Medical Journal,' January 28th, 1899). Probably so markedly dissimilar results with typhoid bacilli from different sources would not be obtained if a lesser degree of dilution was employed.

If, therefore, only a culture of bacillus typhosus from one source is to be used for the general application of this test, one giving the reaction well in the greatest number of cases should be employed.

The age of the culture is of importance. A stock culture is kept, from which young cultures are made as required. What the best age, temperature for growth, etc., of this stock culture should be are matters on which most workers differ greatly, and I cannot enter upon it here. Personally I find a culture on agar from two to six weeks old, kept at the room temperature, answers very well. From this stock culture a tube of sterile broth is inoculated and grown in the 37° C. incubator. All authorities are agreed that this broth culture should be young, not more than twenty-four hours old if grown at this high temperature. In practice we keep, therefore, a stock agar culture, and from this a broth tube is inoculated overnight, put in the 37° C. incubator, and is ready for use next morning.

Now to the performance of the test itself. It can be done microscopically, or by using sedimentation tubes. We will consider the microscopic method now.

The simplest way to perform the dilution is by using a platinum loop. This merely consists of a piece of fairly stout platinum wire fused at one end into a piece of glass rod to act as a holder, while the other end is bent round to form a small loop. Using this to dilute with, you will require the following things:—a young broth culture of bacillus typhosus, two slides each with a depres-

sion in the centre, an ordinary slide, coverslips, a watch-glass, a pot of vaseline with a glass rod in it to act as a brush, a platinum loop, and some inert diluting solution, such as a tube of sterile broth or 0.75 per cent. sodium chloride solution (normal saline).

The first point which I wish to impress upon you is the very great importance of having everything ready before you begin to dilute. Take one of the slides with a central depression and paint a ring of vaseline round the depression. Clean a coverslip and have it ready and handy. Have the ordinary slide clean and dry. Pour a little of the diluting solution (normal saline, or whatever it may be) into a watch-glass. Now, having everything ready, break off the ends of the pipette containing the suspected blood, and blow out the serum on to a corner of the ordinary glass slide. Touch this with the platinum loop, and so take up a loopful of the serum. Deposit this near the centre of the same slide. Sterilise the loop in the flame to get rid of excess. Allow loop to cool, and then place successively nine loopfuls of the diluting fluid round the loopful of serum. Mix well together. We have now diluted the serum one in ten. With care and a little practice approximately equal quantities can be deposited by means of a platinum loop. Take up a loopful of this one in ten dilution and place it near the clean end of the slide. Sterilise the loop and allow to cool. Remove the plug of the broth culture of the typhoid bacillus, and place successively four loopfuls of the broth around the last loopful. Mix well together. At once put a loop of this on to the centre of the coverslip, and lay this on the slide which has the ring of vaseline round the central depression, so that the drop hangs freely in the depression. Carefully see that the vaseline forms a continuous layer between the coverslip and the slide, so that an air-tight chamber is formed in which the bacilli are suspended. The above dilution should be performed as rapidly as is consistent with accuracy to avoid drying up. That is why it is so essential that everything should be ready first. If blood dried on a slide be used, a drop of normal saline is added to it, and rubbed with the blood till of a pale red colour. A loopful of this is taken and treated in the same way as a loopful of serum, *i.e.* nine drops of normal saline are deposited round it, etc. Such a loopful of

blood and saline may be taken as approximately equal to a loopful of serum. I have here diluted the serum one in fifty. In talking of dilutions, it may not be superfluous to add that we always refer to dilutions of the *serum* (or blood, as the case may be).

We have now made what is called a hanging-drop preparation. This should be labelled, and on the label should be written the dilution (here one in fifty), the name of the case or the ward and bed, and the time at which the dilution was made.

Just in the same way, but using normal blood, a control test should be made, *i.e.* with normal blood diluted to the same extent. It is not necessary to do every time a control with normal blood, but in every case when not done, a drop of the typhoid culture should be examined in a hanging drop to see if it is actively motile, and shows no spontaneous clumping.

A small vessel of five per cent. carbolic acid should be at hand, into which the infected slide and other materials can be at once put.

The hanging drop is now examined microscopically. Most people, if not accustomed to examining unstained bacteria, find a considerable difficulty in focussing and finding the organisms. If, however, attention is directed to the three following points, but little difficulty should be experienced.

First, use the one-sixth or one-eighth inch objective, and not the one-twelfth oil immersion. Typhoid bacilli can always be easily seen with the one-sixth. By using this objective they are much easier to find than when higher powers are used, and it has the great advantage of bringing a large number of bacilli into a single field of vision, and so rendering it more easy to see any clumping. Secondly, focus the edge of the drop; when this is found, then move to nearer the centre. Thirdly, it is imperative to cut off a good deal of the light, and as a practical point I always find it easier to work with a bright light, cutting off most of the light by using a small aperture of the diaphragm rather than to use an initially dimmer light, but with a wide aperture.

If diluted as above we have a dilution of the serum of one in fifty. I think that for practical purposes a dilution of one in thirty or one in fifty is the best to use. One in fifty is certainly sufficiently dilute to obviate any fallacy from clumping

by non-typhoid serum, and yet not so dilute but that a well-marked reaction is obtained.

I should like to say here that I think that for clinical purposes—and here I am only speaking of the reaction for diagnostic purposes—it is not advisable to dilute the serum too much. I have found in not a few cases that a serum from a well-marked, definite typhoid fever case will not react to a dilution of one per cent. in two hours with certain typhoid bacilli; yet in a dilution of one in thirty or one in fifty will show a fairly marked reaction. Thus in regard to the case in Ward III I quoted to you just now, you will remember it reacted not at all in one per cent. dilution with two varieties of bacillus typhosus. This was in a very well-marked case of enteric fever. Using one of these varieties, I repeated the test, doing the reaction with a dilution of one in fifty. I examined at the end of one hour. I will read you the note on the reaction as I recorded it. "After one hour all over the field are a number of clumps which are quite well marked but not very large. Bacilli in the clumps show active movement, and bacilli in active movement are scattered all over the field. Two hours—the same." I have marked this, "Positive reaction weak." A control with normal blood was made, and at the end of one and also two hours showed very marked movement and no signs of clumping.

Now usually in the serum diagnosis of enteric fever as ordinarily performed only typhoid bacilli from one source is used. It might easily have been the variety I have been talking about. If a dilution of one per cent. only had been used, then a negative reaction being obtained, the case from this point of view would have been said not to be a case of typhoid fever. But by using a dilution of one in fifty, a reaction was obtained which, if not to be taken as quite positive in its evidence, yet would have led to a subsequent examination in a day or two. Similarly in Ward VIII, bed 9, a case of enteric showed, using five different varieties of typhoid bacilli in each case, a negative reaction in one per cent. dilution; yet two of these varieties when examined, one in one in thirty dilution, and the other in one in fifty dilution, showed weak, positive reactions. I could give other instances. In these days, when it is so much the fashion to recommend a high degree of dilu-

tion, and to brand dilutions of one in thirty and even one in fifty as leading to inaccuracy, it is well to remember that it is possible to so much dilute that the reaction is obscured or not obtained, and we must, for clinical purposes, not avoid the Scylla of insufficient dilution merely to be swamped in the whirlpool of an over-watered serum.

For purposes of research and for theoretical considerations it is a different matter.

Another and more accurate method of dilution is by means of a pipette. This can readily be done. For this purpose the best kind of pipette to use is simply a short piece of glass tubing drawn out at one end into a long capillary tube. The other end is not drawn out. The serum is blown out on to the slide just as I described for diluting with the platinum loop. By touching this with the capillary tube the serum rises in it. There must be no break in the continuity of the column of fluid. Mark on the glass the upper limit of the serum, either with one of the pencils which mark on glass or with ordinary ink. Blow out the serum into a clean, dry watch-glass. Allow the diluting fluid to flow in up to the same mark. Blow this fluid into the watch-glass containing the serum. Do this four times in all. Mix well. The serum is now diluted one in five. Suck up some of this into the same pipette and again mark the height it reaches. Blow this into a second watch-glass. In the same way draw up some of the diluting fluid, *e.g.* normal saline, to level of last mark. Blow this into the second watch-glass, and repeat again four times in all. Mix well. You now have a dilution of one in twenty-five.

Mix in the same way with the pipette equal parts of this one in twenty-five diluted serum and of the typhoid broth. You thus have a dilution of one in fifty. Examine a drop of this mixture in hanging drop as before. Put infected pipette, etc., into five per cent. carbolic acid.

All this sounds very complicated and it takes a long time to describe, but it can, with a little practice, be done very quickly and quite accurately.

Both the diluting and the collecting pipettes can be readily and rapidly made by any one with a few pieces of ordinary glass tubing and a Bunsen burner, or even a gas jet or spirit lamp.

We have a few points still to consider. One is—what constitutes a positive reaction?

As I said at the commencement of this lecture, the changes to look for are cessation of movement and clumping. One may, however, get clumping with a good deal of movement, and also with a good many bacilli between the clumps. The clumping is the most important feature. If the control shows no clumping, but in the hanging drop from the suspected case there is well-marked clumping, even with many bacilli between, and these showing considerable movement, it is a positive reaction, but not so marked as when complete clumping and cessation of movement takes place. Exactly what constitutes a slight reaction would probably vary with different observers, but in well-marked cases there can be no doubt, though, even in these, bacilli between the clumps are frequently seen and may be numerous.

Another very important point is—what time-allowance should be given in which the reaction has to take place? If one in thirty or one in fifty dilutions are used, I think one hour is a fair allowance. A good plan is to examine at the end of a quarter of an hour, and then leave till the hour is complete and examine again. If there are no signs of clumping, then it is a negative reaction. With one per cent. dilutions I always allow two hours, but even then it is rare to find cases showing after two hours' clumping, which at the end of one hour showed no signs of it.

Just a word on possible fallacies from faulty technique. They are chiefly, either using a platinum loop which has not been allowed to cool down sufficiently after sterilisation, or allowing the drop to partially dry up, perhaps before the hanging drop is made, by not having everything ready before dilution; or possibly after it is made, by the layer of vaseline not being continuous and so not forming an air-tight chamber.

So far we have been talking only about the microscopic method of performing this serum diagnosis reaction. There is another and valuable way in which this reaction may be carried out. This is the sedimentation method. Performed by this method no microscope is required, but the reaction is carried out in sero-sedimentation tubes. The simplest form of tube for this purpose is a piece of glass tubing drawn out at one end into a fairly long capillary tube. The other end is not narrowed, but a plug of cotton wool is inserted. This tube is quite similar to the diluting pipette

described above, except that the long capillary tube should be wider. These tubes are very similar to Wright's sedimentation tubes (see 'British Medical Journal,' February, 1898), except that there is no bulb to act as a mixing chamber. The blood is obtained in pipettes as already described. Rather more blood should be taken, and the blood must be collected in pipettes and not on a slide, for when the reaction is performed in this way, no corpuscular elements should be present. The serum is diluted one in twenty-five by means of a pipette, and mixed with an equal quantity of typhoid broth in the same way as I described a few minutes ago. It is best to mix in a clean, dry watch-glass. Instead of making a hanging-drop preparation as we did then, the mixture is allowed to flow by capillary action, or is sucked up if necessary into one of these sedimentation tubes. It is allowed to fill nearly all the capillary tube. By tilting the tube the serum and typhoid broth mixture is made to flow away from the free, narrow end of the tube. This is then sealed in the flame. The tube is labelled and allowed to stand at room temperature in an upright position for twenty-four hours. Just in the same way a control should be done with normal serum or simply with normal saline. The control after twenty-four hours will show a uniform turbidity. In the other tube, if the serum is from a typhoid patient, well-marked plugs are seen at the bottom of the fluid, and sometimes also along the sides of the tube. The fluid above the plug is quite clear if the reaction is complete. It is the presence of these little plugs of agglutinated bacilli, with the clear fluid above, which are the evidence of a positive reaction, and show that the serum is from a typhoid fever patient. As Professor Wright remarks, normal saline and not distilled water should be used for diluting the serum, to prevent possible confusion from sedimentation of flakes of serum globulin.

Some authorities consider this method of performing the serum reaction as valuable, or even more valuable than the ordinary microscopic method. It at any rate requires no microscope, and as has been said, requires no watching, but "performs itself."

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A CLINICAL LECTURE

ON

CASES ILLUSTRATING REPAIR IN CANCER OF THE BREAST.

Delivered at the Middlesex Hospital, February 27th, 1900.

BY

A. PEARCE GOULD, M.S., F.R.C.S.,

Surgeon to and Surgeon in Charge of Out-Patients,
Cancer Department of Middlesex Hospital.

GENTLEMEN,—I have many opportunities of speaking to you about the diagnosis and treatment of cancer of the breast, and of pointing out to you the differing forms under which this disease appears and the variations in its course and progress. The progress in cancer of the breast is generally in a downward direction, from bad to worse, till death comes as a merciful deliverance from the long-drawn-out agony. I want this afternoon to look at quite another aspect of this disease, and to lay before you evidence of repair in cancer; and we shall find this evidence in patients admitted to our Cancer Department as subjects of incurable cancer of the breast, and I think the cases I shall now show you will convince the most sceptical among you that we are justified in speaking of repair in cancer even in its advanced stages. I venture to think, gentlemen, that such a demonstration as I am able to give you this afternoon is not only of interest, guarding you against a prevalent error, but that it is of the very highest practical importance, for it not only throws light upon the nature of cancer, but it justifies—nay, compels—a belief in the possibility of the cure of cancer, and gives us an indication of the direction in which a cure is to be sought. In the natural history of cancer there is very much to depress and discourage us. It is a welcome change to study the one bright spot in the otherwise dark picture, and to get from this study inspiration and hope that even for cancer our science and art will be able to find a preventive or a remedy.

The evidence of repair that I show you to-day is threefold—(1) *the epitheliation or cicatrisation of cancerous ulcers*; (2) *the disappearance of small nodules of secondary cancer*; and (3) *the total disappearance of extensive masses of cancer*.

1. My first point is that repair is manifested in *the epitheliation or cicatrisation of cancerous ulcers*.

The first case I show you is that of an old man from Greenhow Ward. The patient is eighty-six years of age, and has a remarkable history. Twenty years ago he was in the London Hospital for an epithelioma of the lower lip; it was re-

which you can all see quite plainly, measuring half an inch in one direction and one third inch in another, covered with epithelium. He has only been in here a short time. Five days ago—that is on last Thursday—I saw that island. It is larger to-day than it was then, and I have hopes that we shall see taking place in this ulcer a distinct spread of the epithelium over the surface, and let us hope we may witness complete repair; but that is too good a result to expect (see Fig. 1).

To illustrate the same fact, I show you these drawings, which were made of cases in Laffan



Fig. 1.

moved, and you see the scar in the middle line. Unfortunately it was not "microscoped," but there is little doubt that it was an epithelioma: the disease is so characteristic that accuracy in its diagnosis is a matter of some certainty. He has had no recurrence of that affection; but now, twenty years later, he comes in here with a rather advanced scirrhus cancer of the right breast, the surface of which is extensively, though not deeply, ulcerated. In one part of the ulcer you will see epitheliation taking place. Close to the deepest part of the ulcer there is a little island,

Ward last year. One is a drawing of a patient who, some months ago, came in with advanced malignant disease in the left breast, on whom no operation had been performed. The large ulcer depicted was the effect of the disease only, and she had a great mass in the axilla and another above the clavicle, causing œdema of the arm. In the middle of this large malignant ulcer, occupying the position of the left mamma, there are several islands of epitheliation; and that process of repair went on under our observation, though all the time the patient was going downhill, and she

eventually died. This drawing was taken a few weeks only before her death (see Fig. 2).

This second drawing was taken at the same time from the patient lying in the adjoining bed. This woman came in with advanced disease in the right mamma. No operation had been performed; the right breast had been entirely and cleanly removed by the disease itself. When admitted, in addition to numerous secondary deposits in the opposite mamma, in the skin, muscles, and bones of the chest, and in the liver, she had a large ulcer occupying the position of the right mamma, with the edge showing a white line. We watched that white line gradually advance over the cancerous ulcer, just as healing occurs over a simple ulcer,

will come upon the scar of the ulcer. You will notice that the epithelium covering the healthy skin and the scar is identical. You will see also that below the epithelium, which can hardly be distinguished from the epithelium of the normal skin, there is present fibrous tissue, and in among that you will see distinct remains—characteristic remains—of the original scirrhus growth. You can see under the microscope, over the top of actual scirrhus tissue, a new growth of squamous epithelium. I have not seen in any other case such an extreme repair of a large ulcer as shown in that drawing. In these three cases, then, we see three stages of this cicatrisation of cancerous ulcers. In the man we have its first commence-

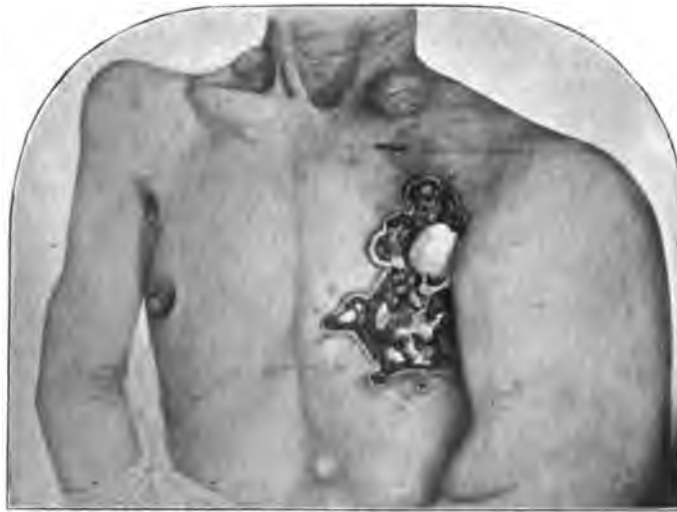


Fig. 2.

until the whole was covered over by a scar indistinguishable to the naked eye from the ordinary scar of a wound or common ulcer. I had this drawing made when the scar was complete. Within a month of the drawing being taken the woman died; the disease was found to have spread into the lungs, and she also had extensive deposits in the liver. The disease, therefore, was running its usual course, but in one place in this ulcerated surface there was shown this very remarkable repair (see Fig. 3).

Mr. Clifford will show you under the microscope a section of the scar made by Dr. Campbell Thomson. In that section you will see on moving the slide, first a piece of the normal skin, and then you

ment, not at the edge, but in the centre of the ulcer. We saw in one of the women large islands of scar tissue in a great cancerous ulcer, and in the other the healing over of the whole of a malignant ulcer by a scar which gradually spread in from the edge.

The occurrence of cicatrisation in cases of rodent ulcer has been long known, but there are so many points of difference between rodent ulcer and cancer that I exclude it from our consideration, and speak only of scirrhus of the breast. I want you to notice that this epitheliation does not take place only in patients who are doing well, but that we may see it in advanced stages of the disease, and even shortly before the fatal termination of a case.

2. The next point I want to illustrate is a rather more marked form of repair in malignant disease, a repair amounting to *the shrinking and disappearance of malignant nodules*. I show you here an example of it in this woman. In 1896 she was a patient in the London Hospital, and had her left breast removed for scirrhus. She came to me in the cancer department in 1897 with very numerous secondary nodules in and around the scar. She was otherwise in good health, and I thought I would try if very free removal would do her good, so I made a very wide excision of these numerous nodules in and under the skin, cutting six inches in each direction, and then covered the raw surface with Thiersch grafts. It was a simple

undoubtedly secondary scirrhus nodules, have got smaller, and some of them have disappeared. I cannot, of course, show you the disappearance, but there was a nodule in this spot here, where it is now quite soft, and there is nothing to be seen but a little stain in the skin. At its lower part you can just feel a little induration; some weeks ago that was a thick nodule. Here are two nodules which have got smaller, and here one is undoubtedly flatter and smaller. This fact, the disappearance of these secondary nodules in some cases, has been observed many times, and some surgeons have referred to it. I do not tell it to you as any new thing; all I want to impress upon you is that while we generally regard malignant

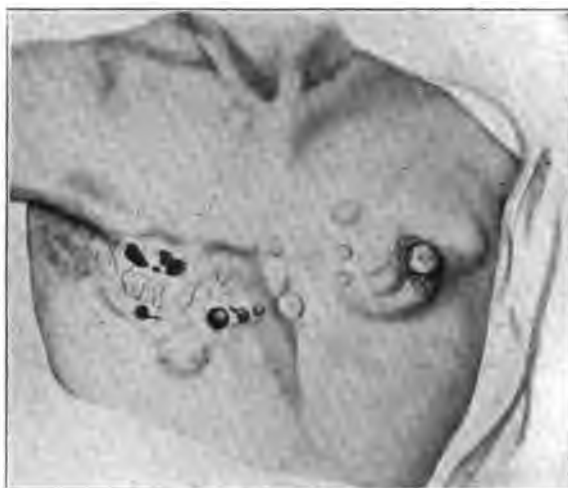


Fig. 3.

matter, and all went well, and she remained well for some time. That was in March, 1897. In February, 1899, she came back with a malignant growth in the right, the opposite breast; a tumour of some size, quite characteristic in its clinical features, occupied the central part of the breast, and the glands in the axilla were enlarged. I then removed the right breast and cleared out the axilla. Soon after that operation she showed some nodules in the skin just outside the scar of my first operation, and some of these nodules you can see now. I have been seeing her frequently ever since last February, and what we have noticed about this patient, and what she has noticed herself, is that some of these nodules, which were

disease as running one course only, yet there is evidence to show that malignant disease is capable of undergoing a certain amount of repair; and when, as in this case, we see nodules getting smaller and disappearing, we have a striking proof of this statement.

There is another interesting feature in this case that I must point out to you in passing. When she came in last summer we noticed that the right side of her chest was distinctly smaller than the left, and that it was not moving at all with respiration, and we found it was dull. Dr. Fowler kindly examined her two or three times, and he confirmed these observations. It is his opinion that this patient is suffering from secondary malignant

growth in the right lung. For five months she has exhibited all these signs of malignant disease in the right lung. She does not show in her face any great indication of the serious state she is in; she suffers no discomfort except when walking about, and then she soon gets rather short of breath, which is natural enough considering she has but one useful lung. It is a striking instance of the slight symptoms to which extensive secondary malignant disease of the lung may give rise. Her cough is very trifling, and she has never had any prune-juice expectoration; indeed, she has very little expectoration of any kind.

3. Thirdly, I come to the most striking and most important evidence of repair in cancer—the *total disappearance of extensive malignant growths*. This may be either “spontaneous” or follow upon treatment. As an example of the spontaneous “cure” of cancer, I must remind you of a case that was in Laffan Ward some time ago. I have already mentioned this case in a clinical lecture earlier in the session, but the case is so remarkable and so germane to the subject we are considering this afternoon that I make no apology for repeating the main facts of it now.

A woman was admitted with advanced recurrent malignant disease of the right breast, secondary deposits in the left breast, and the usual evidences of carcinoma of the right lung. The neck of her right femur underwent spontaneous fracture in such a way as to make it reasonable to suppose that there was a secondary malignant growth there also. Thus she had all the evidences of advanced and wide-spread malignant disease. Without any special treatment that patient, while in the cancer ward, got perfectly well, all the nodules about the scar on the right side (where operations had been performed before she came here), all the nodules in the left breast, and all the signs of malignant disease in the lung disappeared. The fragments of her right femur united firmly, and though, it is true, there was some shortening of the limb, still the leg became strong and useful. A few months ago I saw her, and she was walking with a limp, but otherwise was quite well; yet this same woman for some weeks had been daily expecting death from cancer. That is an old story, and I refer to it again especially because it was a *spontaneous* recovery, no operation was performed here, nothing but careful nursing of the patient to relieve, if

possible, the agonies of what appeared to be her last days. Nevertheless, from the very gate of death, so to speak, that woman returned to health suddenly she began to mend, and recovered. This case is fully recorded in the Clinical Society's ‘Transactions,’ vol. xxx, p. 205, and vol. xxxii, p. 272.

The next case I show you is possibly one in which you will take more interest still. It is that of a patient who is exhibiting as far as we can tell a very rapid improvement—I do not like to use the word “cure”—as a result of treatment. She is a single woman forty-one years of age, who was admitted into Laffan Ward with apparently incurable cancer of the mammæ. Two and a half years ago she noticed a small lump in the right breast; it gradually got bigger, and she consulted a homœopathic physician, who treated her in a certain way, which was, he said, to “disperse this growth.” The growth steadily increased, then the surface began to ulcerate, and so she rather lost faith in that practitioner, and was advised to come here, and was admitted into the cancer ward. When she came in she was thin, and she said that she had got thinner; but she had not been weighed, it was only her impression. There was a firm malignant infiltration involving the whole of the right breast and of the skin over it. The breast was fixed to the underlying ribs. Above the nipple was an irregular ulcer measuring three inches at its longest border. There were several cancerous nodules in the skin around the breast. In the right axilla were several hard, tender, enlarged glands; the movement of the right arm was very limited, and there was œdema of the right arm and forearm. In the left breast was a hard, slightly tender mass on the outer side, the size of a large walnut, freely movable over the ribs. There were hard, tender, enlarged glands in the left axilla, also some glands above the right clavicle and under the sterno-mastoid. There was an area of venous congestion below the tumour on the right side from the lower border of the left chest to the right axilla; another area of the same nature was also to be seen above and to the outer side of the malignant infiltration. The ulcerated area measured one and three quarter inches by two and a quarter across the middle, the corners being a little prolonged, and the inner border being three inches long.

To recapitulate, the whole right mamma was infiltrated with obvious malignant growth, and there were secondary deposits in the other breast, in the axillary glands on each side, and in the right supra-clavicular glands. There was scarcely any movement at the shoulder-joint, and the arm and forearm were œdematous.

This patient is forty-one years of age, and as the climacteric has not been reached, I thought that if ever there was a case which was suitable for oöphorectomy and thyroid feeding this was one.

On January 31st, 1900, I removed both ovaries and tubes, which appeared quite healthy, and one week after the operation I ordered thyroid colloid tabloids, first one a day for three days, then two a day for three days, and since then three tabloids a day. You can see for yourselves that there is a very marked change in the local condition, so that no one could now describe the case in the way it is described in the hospital records of a month ago.

The ulcer has, with the exception of a place the size of a sixpenny piece, disappeared, and a month ago it measured three inches in length. The induration was as hard as a board; now you can see the surface is softer and supple, I can move the skin over the ribs, whereas before it was fixed. Of the nodules formerly scattered about most have disappeared, and those that remain are thinner and smaller. The areas of deep venous congestion are almost invisible now, nothing approaching to what they were. In the right axilla the formerly enlarged glands can no longer be felt at all, nor can we now feel glands under and behind the sterno-mastoid muscle. On the left side there was a mass the size of a walnut in the outer part of the breast, and all that one can now say of it is that there is one tender hard nodule in the axillary margin of the gland as large as a pea. There are no glands to be felt in the axilla. Concurrently with the absorption of the diseased glands in the right axilla the œdema in the arm has vanished, and the patient is now able to feed herself, a thing she had not done for three months.

Within a month this most striking change has taken place. As soon as we witnessed the striking change that was occurring in this case it became necessary, if possible, to obtain anatomical evidence of the nature of the growth, so, with the patient's consent, Mr. Clifford excised one of the

nodules a few days ago for microscopical examination.* That is the explanation of the collodion dressing you see here. Day by day one can see the improvement in this patient's condition. She still has considerable limitation of movement in the arm, because the infiltration of the pectoral muscle has damaged it, and though the malignant disease undergoes repair it is not possible to restore the lost muscular tissue. I fear, therefore, that she will not get back the full use of the arm.

This case differs from the others inasmuch as one must attribute the change that has taken place to the treatment that has been pursued. Oöphorectomy was first suggested and practised by Dr. Beatson, and has been carried out by some other surgeons and physicians. Dr. Herman has published four cases in which he has employed this treatment; Mr. Stanley Boyd, too, has practised it, but I gather that he has not much faith in the thyroid feeding. I have operated four times for otherwise incurable cancer of the mamma, and I have followed it up with thyroid food in the last two cases. In the first two I could not see the least indication of improvement.

The third case was operated on last summer. Up to the time she left hospital there had been no marked improvement, as in the case you have just seen. I have not heard of her lately, so cannot speak of the ultimate result in her case.

I do not wish just now to discuss the therapeutic value of the treatment to which this patient has been subjected, or to attempt to set a value on its two constituents. I want only to fix your attention on one point, *the fact of repair in cancer*. A recognition of this fact will not only affect our views of the true nature of cancer, but it will act as a constant stimulus to us to find out some method of treatment, some therapeutic agent, for this disease which will lead to this repair. If we go on with dogged incredulity as to the possibility of cancer undergoing cure we are never likely to find a cure for it. Results are very much according to one's faith; if you have no faith in the possibility of cancer being cured it is not to be hoped for that you will see any cure for cancer. But if you will really grasp the evidence that nature gives us that this disease is curable, you will be always alert to find out some method of dealing with it.

* It was found to have the structure of scirrhus.

Before leaving these cases I wish to draw your attention to two points of some interest. First notice that these cases have all been taken from our cancer wards. The case I spoke of which underwent spontaneous cure, and the remarkable case of healing over of a large cancerous ulcer, were observed in a ward which had been inhabited for many years by patients suffering from advanced malignant disease. If there is anything in the infectiveness of cancer, if it can infect houses and rooms, as some say, then the old cancer wards of Middlesex Hospital would seem to be the very last place for the reparative power of cancer to manifest itself; yet it is in patients from these very wards that I produce to you to-day these evidences of repair in cancer. Secondly, it is worthy of notice that I have not shown you repair in slight early cases of the disease, which have not advanced far, and which have not affected the constitution of the patient, but all these patients have been the subjects of advanced cancerous disease, two of them were very near their end when the local repair took place; in another case death seemed to be impending, the condition reached its very worst, and then repair and local recovery were seen.

Before you go I want to show you one other case of cancer of the breast, to illustrate quite a different point, and yet one that is probably in some way related to this interesting fact of repair.

This patient is an elderly woman, seventy-four years of age. She was first in this hospital in 1862, when Mr. Nunn removed her right breast for scirrhus. Mr. Nunn watched the patient, and in his recent little book on cancer he refers to her. He saw her ten years after the operation, and there was no sign of recurrence. Then, six years later (1878), Mr. Nunn found a recurrence of the growth in the patient's axilla. The lump then seen has gradually advanced, and I want you to notice how limited is the disease now, even after twenty-two years of growth. The cancerous growth measures about five by three inches; it is raised above the surface, is irregularly ulcerated in the centre, and is still freely movable over the ribs. The contractile nature of the growth is shown by the puckering in of the skin around it. At one side you notice the edge is well raised and rounded, almost tuberous in appearance. Above the clavicle is a firm growth in the glands, which is quite fixed to the muscles and first rib. Beyond this we have been unable to

detect any evidence of secondary cancerous deposits. I want you to notice about this extremely chronic secondary cancerous growth that it does not partake of the characteristics of atrophic scirrhus. I show it to you to-day to illustrate to you how *exceedingly chronic secondary recurrence after scirrhus of the breast may be*, though usually a rapid form of malignant disease. This we may regard as the result of the power of living tissues under certain unknown circumstances to hold in check the natural progress of cancer; and it is from this standpoint that I want you to view this case, and to see in it additional reason to hope that we may discover some constitutional means of dealing really satisfactorily with this fell disease. The case also illustrates that *long freedom of recurrence is not the same thing as cure*. Some have thought that if a patient remains free from recurrence for three years after operation they may regard her as cured, but it is nothing whatever of the kind. Here is an illustration of late recurrence, and I could tell you of many others quite as striking. You can never speak of a patient as cured until she has run the whole course of her life without any sign of return of the malady. But while there is no time limit that we can fix, every year that passes without recurrence adds to the prospect of permanent freedom from the disease.

BUCHNER calls attention to the surprisingly good results that follow brushing of the teeth and the gums with 45 per cent. alcohol twice daily. He reports striking results obtained with alcohol dressings in the treatment of articular tuberculosis. He advocates alcohol dressings in the treatment of syphilitic gummas, on the score that the ferments in the serum not only destroy the specific germs, but also hasten the removal of the pathologic products. Laryngeal tuberculosis and peritoneal tuberculosis are also mentioned as suitable conditions for alcoholic dressings. Inhalation of alcohol in phthisis and alcohol gargles in angina and diphtheria are suggested. It now remains to test, in clinical practice, the theory of the curative action of hyperæmia produced by the application of alcohol.—*Journ. A. M. A.*, April 7th, 1900.

A CLINICAL LECTURE ON CANCER OF THE UTERUS.

Delivered at the London Hospital,

By **Dr. HERMAN.**

GENTLEMEN,—I have four cases in the ward about which I wish to speak to-day. The first is that of a woman aged forty-four, who has had six children, the last born dead four years ago. She was regular in her menstruation until twelve months ago, since which time she has been losing continually. For three months she has complained of pain in the abdomen and down the right leg. For two years she has had a discharge, and during the last twelve months this discharge has been blood-stained and offensive.

The next case is that of a woman who has had four children, the youngest two years ago. Her complaint is that she has been feeling ill and weak since November, 1898. She was regular until three weeks ago. Since May last she has had a blood-stained discharge, and at times this has been offensive. She complains of weakness, and has for nearly a year been losing strength. She has been treated for anæmia for four years at a hospital. I should say that the treatment was not hospital treatment in the true sense of the word, for she only went now and then as a casualty patient. Still, she has been seen, on and off, for four years, but has had no local examination. She has had pain, but never very bad. She has got thinner recently. When she was further questioned about menstruation, it appeared that she had regular periods once a month, lasting for three days, scanty in quantity, with no pain, and she has not seen more discharge recently. You will see that this patient's complaints differ from those of the first one. This one's main complaint is of wasting and discharge; hæmorrhage seems hardly to be complained of.

The next case is that of a woman aged thirty-nine, who has had ten children, the youngest five years ago. This patient thinks she was quite well in April last. She was regular until six months ago, when she had a "flooding," lasting six weeks. She says her bleeding was severe. She has had a watery discharge for six months; it was offensive in odour at first, but not lately. She complains of weakness

on the left side. She has lost much flesh. Her hæmorrhages, weakness, loss of flesh, and discharge appear to have dated from about the same time.

The fourth case is that of a woman aged sixty-six. She has never been pregnant. She complains chiefly of pain, which started a year ago. Lately the pain has been more continuous, so that it keeps her awake at night. She left off menstruating at the age of fifty. Two years ago she had a discharge of a dirty colour, sometimes a little red.

All these patients complain of the same broad symptoms — hæmorrhage, pain, discharge, and wasting, the cases differing from one another in the relative date at which the symptoms began. In one the symptoms came at about the same time; in another the hæmorrhage preceded by a long time the pain and discharge; in the third discharge preceded both pain and hæmorrhage; in the fourth hæmorrhage was the first symptom.

These patients are all suffering from cancer of the uterus, and the symptoms are those with which cancer of the uterus is usually accompanied. In my experience bleeding is usually the first symptom. There are two popular fallacies to which I would call attention which are important because they often lead to the neglect of proper treatment in cases of early cancer. One of them is that change of life is normally and naturally accompanied by hæmorrhage. The public have the idea that there is something in menstruation which is beneficial, and that when menstruation becomes irregular, an occasional excessive hæmorrhage is rather a good thing and is natural. Even with medical men there is sometimes an idea that such hæmorrhage is natural. As far as my knowledge goes there is no such thing as a normal excessive hæmorrhage accompanying the change of life. Women at the change of life are liable to diseases of the endometrium, and hæmorrhage at that time does not always mean carcinoma, but it means something abnormal, and in any patient, especially one who has had children, an excessive hæmorrhage at the change of life ought to call for local examination. Indeed, any excessive hæmorrhage in a parous woman, whether at the change of life or not, ought to call for local examination. There is no hæmorrhage which can be assumed to be natural and not to require treatment. It always requires examination, and sometimes requires treatment.

Another popular fallacy is that cancer of the

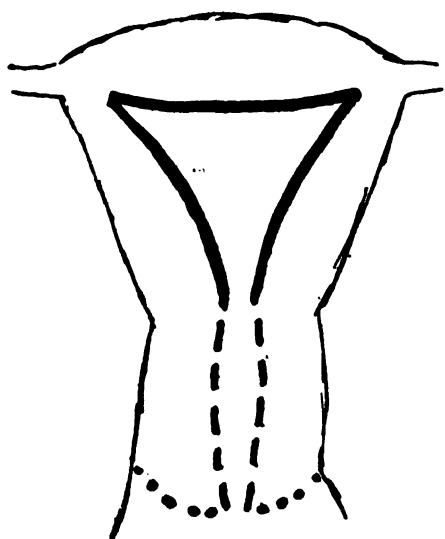
uterus is always a painful disease. Patients who suffer from hæmorrhage and discharge sometimes think that because there is no pain the disease can be nothing serious; and sometimes medical men think that because there is no pain the disease cannot be cancer. Some cases of cancer go through their whole course without pain, and the pain of cancer when present is not always severe. In only one of these four patients is the pain so bad as to keep her awake at night.

Another fallacy is that the discharge of cancer is always offensive. It is only offensive in the later stages of the disease, and then the offensiveness arises from the admixture of broken-down tissue which has sloughed off from the growth. It is the decomposing particles which make it offensive, and the discharge is not offensive until the cancer begins to break down. The fact that a discharge is not offensive is no evidence that the disease is not cancer, and is no reason for assuming that cancer is not present.

I am going to speak of cancer this afternoon simply from a clinical point of view. Taking the cases in the long run, wasting is a regular sign of cancer; but the wasting at the time we are consulted is not always great, and many causes besides cancer lead to wasting. Therefore it does not follow that because a patient is wasting she has cancer; and even where cancer is present the subjects of it may for a time gain flesh. Indeed, I have seen cases of cancer so far advanced that it has been impossible to remove it, and yet the patients had not lost flesh, and were fat. Thus absence of wasting is no proof against cancer, nor is the fact that a patient is thinner than she used to be any evidence of the existence of cancer. The bleeding in cancer comes from breaking down of the growth. It is one of the characteristic features of cancerous growths that they tend spontaneously to break down and bleed on the slightest provocation. Sometimes the examination by the finger in a cancerous case will provoke bleeding. If you even wipe a cancerous cervix with a piece of cotton-wool it will bleed; and it is common for patients to tell you that marital intercourse causes bleeding. If you are told that, it is very suggestive of uterine cancer. Such bleeding may also be produced by a mucous polypus of the cervix; but bleeding on contact is a frequent and early symptom in cancer, and therefore important. When any of the sym-

ptoms I have mentioned are present an examination should be made without delay. When you make a local examination, what you have to look for is a new growth which tends to break down. In cancer of the cervix the part is enlarged by the new growth, and the growth itself bleeds very easily. If it is an early case you will see spots of ecchymosis or ashy-grey spots of sloughing, these signs indicating the breaking down of the growth. The growth of early cancer on the cervix does not present the uniform bright red colour of an erosion, but it is mottled with spots and ecchymosis, white or grey sloughing tissue, which bleeds on slight contact. Fibroids sometimes disintegrate. We have had one in the hospital recently which began to disintegrate; but the disintegration of fibroid is a non-vascular disintegration; there is a stringy fibrous mass which is not vascular, and which does not bleed on contact. When cancer is advanced the disintegration goes on fastest at the central or oldest part of the growth; therefore in an advanced case of cancer you get a funnel-shaped ulcer with thickened raised edges, the raised edges being the more recent parts of the growth, the funnel-shaped ulcer representing the older part, where the growth has broken down. The cancer, as it grows, infiltrates all tissues; it derives its name—cancer, or crab—from the way in which it invades by claw-like processes the surrounding parts. It fixes the uterus not only from invasion of the adjacent parts by the growth, but because, when the cancer has advanced, it causes peri- and parametritis, and the uterus is fixed by that as well as by the actual extension of the cancer. When you get a funnel-shaped cancer, with fixation of the uterus, the case is too far gone for attempting removal. In the first patient I mentioned this was the case. She had had six children, the last born dead four years ago. Her history goes back for two years. First she had discharge without hæmorrhage, but pain has been present only four months. She has been losing flesh for the past six months. She went recently to another hospital, and was there told that she could not have any operation done. When she came in there was nothing felt on abdominal palpation. On vaginal examination blood was found about the vagina, the cervix uteri was thickened, and so were both broad ligaments. There was an excavating ulcer over a portion of the vaginal cervix and on the anterior vaginal wall

There are various classifications of cancer, and there is difference of opinion as to the way in which cancer begins—a difference of opinion which is due to the rarity with which cancer is seen in a very early stage. The classification of cancer which is generally accepted was first introduced by Drs. Ruge and Veit, and it has been supported in this country by Sir John Williams. According to this there are three kinds of uterine cancer. Cancer affecting the neck of the womb may begin either (1) on the vaginal portion, that is on the mucous membrane between the os externum and the insertion of the vagina—cancer of the vaginal portion; or (2) in the cervical canal—cancer of the cervix. The third kind is cancer of the body.



Part affected in cancer of body ————
 Cancer of cervix
 Cancer of vaginal portion

An able writer on the subject has pointed out that at the circle of the os uteri externum there is a band of transitional epithelium where the epithelium of the vaginal portion passes into that of the cervix. He says that he has investigated this question of the origin of cancer very carefully, having sought for cases of early cancer, and has come to the conclusion that cancer always begins at this surface of transitional epithelium. He has not yet discovered any case beginning at the cervical canal or at the vaginal portion. If I go back on my own experience it is much the same in this respect, that it is rare to find any case in which

the os externum is not involved; but I think this is because the cases are not found early enough.

The cervix is generally torn in labour, and this makes it difficult to define the exact position of the external os. That is important, because it bears on the view that cancer begins on the external os.

There is one case described by Sir John Williams where cancer seems to have begun on the vaginal portion. The earliest case I have seen had involved the external os and some adjacent mucous membrane. In one of the cases, the notes of which I have read to you, there was a growth which projected into the vagina, and consisted of two masses, one in front and one behind, and between these masses there was a deep ulcerated depression. This felt like disease of both lips of the os uteri, a mass on the anterior lip and a mass on the posterior lip. When removing it I was struck by the difficulty I found in getting into the uterine canal. I could not make out how it was I missed the uterine canal. When the uterus was outside the body and examined I found that the os uteri was behind this tumour, and that the cervical canal was unaffected by the disease. The disease extended just to the os uteri. If this began at the os uteri it was strange that a large growth like that should spread in one direction and not to the slightest degree in the opposite direction. That is unusual in cancer. My conclusion is that the disease began on the anterior lip of the vaginal portion. I have seen several other cases in which, when the patients were examined by the finger, nothing amiss was felt, and when they were examined with the vaginal speculum the vaginal portion appeared healthy; but when the os externum was enlarged by tents or dilators the finger came at once into an excavated cavity, which represented the cervical canal. I find it difficult to believe that in such cases the disease can have begun at the circle of the os uteri and not extended downwards in the slightest degree. I think the disease began in the cervical canal, and for these reasons I am not prepared to adopt the view I have mentioned. I admit the correctness of the observations, but I think they have been misinterpreted.

This classification introduced by Ruge and Veit is important, because it corresponds to important clinical differences. Cancer beginning in the cervical canal has two peculiarities. First, you

cannot detect it until comparatively late in its progress, because the vaginal portion is not affected. The cervical glands extend some distance into the tissue of the cervix, hence this form of cancer is near the broad ligament and early grows into it, so that it cannot be removed. For these reasons cancer of the cervix is the most unfavourable kind, because it is difficult to recognise it early, and because it soon extends beyond the possibility of removal. Cancer beginning in the vaginal portion is the reverse; it can be found out as soon as the patient is examined. If there is a warty growth it can be felt, and if there is discoloration it can be seen with a speculum. Therefore this form of cancer can be diagnosed early. There is a considerable distance separating it from the broad ligament, and it tends to spread along the surface. Therefore it may grow large before it becomes incapable of being removed. In the case I have described the anterior lip was three times the size of a normal cervix, and yet the uterus was not fixed. The uterus was removed, and there is reason to believe that the whole disease has been taken away. Therefore cancer in this position remains capable of removal for a very much longer time. These clinical differences are of great importance.

There is a microscopical classification of cancer of the uterus. Some are squamous-celled, some are columnar-celled. But between these forms of cancer there are no important clinical differences that I am aware of. And in my judgment the clinical evidence of the presence of cancer is far more important than the microscopical. There are several reasons for this. If you scrape away a piece of growth and examine it microscopically, and find it to be cancer, that is evidence of great value; and if conversely, after scraping a piece of growth away and submitting it to the microscope, you could say that the disease was not cancer, then this evidence would be of the utmost value. But that is not the case. If you scrape away a fragment of growth or uterine mucous membrane and do not find evidence of cancer in it, that does not prove that the patient has not got cancer, because there may be cancer elsewhere which you have not scraped away. Supposing you have a growth which occupies the uterine cavity, or projects from the os uteri, and you take away a piece and examine, and find it shows nothing but healthy

glandular tissue, that is not proof it is not malignant. Malignant adenoma runs the course of malignant disease, although the microscopical appearances are not those of malignant disease, but only those of healthy glandular tissue. So that while microscopic evidence that a scraping is cancer may be taken to be correct, negative results from the microscope cannot make sure that the disease is not cancer. It is recommended that in order to determine the malignant character or otherwise of disease of the vaginal portion, a piece of it should be cut out and examined microscopically. This is correct, but cutting away a piece of the cervix is in itself a partial operation, and it is better if you do anything to cut away the whole of the disease, and if it turns out to be cancer you have the satisfaction of knowing that you have removed it. On the other hand, if the disease turns out not to be cancer the vaginal portion is not an important organ, its removal is not an operation which leaves the patient any the worse, and removal of the whole of it is not attended with greater danger than removal of part. Moreover, if the disease is of such a doubtful character that its nature can only be determined by removing a slice of the vaginal portion, it will generally be so limited in extent that the whole of it can be removed without a large operation. These considerations apply to the value of microscopical examination when conducted with every appliance by skilled section-cutters and skilled observers. But when you think of the application of these things in general practice, the opinion of a man who only seldom examines sections of a uterus is not very valuable, and for a man in general practice to keep himself conversant with the most recent histology is a practical impossibility. The diagnosis of cancer in family practice must be made without the microscope. It would be absurd to disparage the microscope in medicine, yet its utility in the diagnosis of cancer in these parts is limited. I think you will hardly ever find cancer diagnosed with the microscope which cannot be diagnosed without it, and if there is good clinical evidence of the presence of cancer, it is unsafe to set aside that evidence because there is doubt about the histological appearances.

Cancer of the vaginal portion begins in one of two ways: (1) below the surface as a flat discoloured spot. If it begins in that way as a

growth below the surface, as a discoloured spot, it remains in that condition only for a short time ; it soon breaks down and forms a shallow ulcer. When it begins below the surface it has a peculiar feeling, similar to moving the fingers over wet india-rubber. This sign should cause great suspicion of early carcinoma. (2) It sometimes begins, more often, I think, as a firm, but friable, warty growth. When cancer begins in the cervix, either in the vaginal portion or in the cervix proper, it seldom extends to the bottom of the uterus. In our museum there are specimens of advanced cancer of the cervix, in which the bladder is eaten into and the vaginal portion destroyed, but in which the body of the uterus is hardly attacked at all.

When cancer extends into the broad ligaments the part which it affects earliest is the cellular tissue running into the utero-sacral ligament ; commonly one side is affected more than the other ; but sooner or later the other side also becomes implicated. Some think it is not implication of the cellular tissue, but of the lymphatics. I do not think that is the case, because in almost every case of cancer the cellular tissue becomes involved, but the lymphatic glands are only involved in a small number of cases of cancer ; they are involved in cancer of the uterus less often than in any other kind of cancer. If cancer spread along the lymphatic vessels one would expect to find the lymphatic glands diseased, but they are not.

I mentioned that the tissue about the internal os seems not very amenable to the growth of cancer. Cancer of the cervix has difficulty in invading the uterine body, and therefore it results that often when there is cancer there is a good deal of thickening in the neighbourhood of the internal os, yet the body of the uterus is hardly attacked at all. When that is the case there may be retention of blood or discharge in the uterine body.

We know next to nothing of the causes of cancer, as I need hardly tell you. Women are much more liable to cancer than are men, and that especial liability is due to cancer of the uterus and cancer of the breast. Cancer of the uterus is three times as common as cancer of the breast, and the importance of these organs in the direction of cancer is shown by the fact that up to fifteen there is no

difference in the relative liability of the sexes to cancer. The great difference is between forty-five and sixty. After fifty there is a less difference between men and women in this respect than before that age. It is said to be much commoner in the poor than in the rich. No doubt that is so, because the poor are much more numerous than the rich ; but I know of no evidence to show that it is proportionately more common amongst the poorer classes than among the well-to-do. It is said, though I know not on what amount of fact the statement is based, that it is less common amongst negroes than it is amongst white women, and less common amongst Orientals than amongst Europeans. There is no question that the liability to cancer of the cervix is increased by child-bearing. Cancer of the cervix uteri is rare in virgins, and is also rare in nulliparæ. The highest proportion of sterile women among patients with cancer of the cervix is 5 per cent., and the proportion of virgins still less than that of sterile married women. We have obviously here an instance of the greater liability of parts to suffer from cancer where there has been local damage. This is exemplified in chimney-sweeps' cancer, and in the liability of smokers to cancer of the lip. So it would seem that the liability to cancer of the uterus is in proportion to the amount of stretching, bruising and laceration to which the cervix is exposed in child-bearing. Laceration of the cervix has been blamed as a direct cause of cancer. It has been proposed to sew up every laceration of the cervix, to prevent cancer. Now while the liability of the cervix to cancer is due to its being injured in childbirth, it does not follow that tearing is the only injury ; it may be the compression, bruising, and stretching of the cervix, and the laceration of the cervix may be beneficial in preventing cancer, by favouring quick expansion, and so lessening the amount of stretching and bruising to which it is subject. If the tearing of the cervix were the special injury favouring cancer, one would expect to find cancer beginning in the apex of the tear. Sir John Williams investigated this, but found not a single case in which, where the cervix was torn, the cancer began in the angle of the tear. If cancer of the uterus does not begin in the angles of the tears, then it is obvious that nothing is to be expected from stitching up the cervix to prevent the occurrence of cancer.

The diagnosis of cancer rests on the discovery of enlargement of the cervix uteri, *plus* evidence of breaking down. Enlargement of the cervix is obvious as soon as it is examined. Sight and touch give sufficient evidence. The evidence of breaking down consists partly in what one sees, and partly on the results of interference with the growth. What you can see depends on the existence of hæmorrhages on contact, spots of ecchymosis, and ashy-grey sloughs. In cases of doubt, Professor Sinclair has spoken in strong terms of the value of the curette; and I support his view to the full. His opinion is that if there is one feature in favour of a growth being cancer more distinct than another, it is the friability of the growth. A moderately sharp curette will easily dig out a lump of cancerous tissue; if you take a spoon you can dig out a spoonful of cancerous tissue. But if you apply a curette in the same way to a healthy cervix, fibrous tumour, or an erosion, it may make it bleed, but you will get either only a gelatinous film or nothing at all away. The possibility, then, of digging out a lump of tissue with an instrument not possessing a cutting edge is one of the most valuable signs of cancer, and one which I think is indubitable.

Pruritus Ani.—J. P. Tuttle, in the 'Medical News' of January 27th, 1900, says he is not a believer in pruritus ani essentialis. The constitutional condition underlying this symptom he recognises as an important element, but insists that there is always an existing cause for the disturbance. This should be earnestly sought and relieved at the same time; where apparent causes are found, such as fissure, fistula, hæmorrhoids, and condylomata, the removal of these may not remove the symptom. Hot water is usually acceptable, but sometimes cold is more so. Carbolic solutions, black wash, salicylic acid, chloral hydrate, chloride of calcium, extract of conium, extract of strontium, camphor, cocaine, etc., all have their uses. A combination upon which he relies more than others is carbolic acid 10 to 20 parts, salicylic acid 2 to 10 parts, boric acid 5 parts, glycerine or cold cream 100 parts. He has used ichthyol with benefit when there is pain. The judicious application of such treatment he believes will relieve nearly all cases, and will make it unnecessary to resort to curetting, actual cautery, or the dissecting away of the itching surfaces.

Medicine, April, 1900.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXII.

GENTLEMEN,—I now go to another subject, which I shall touch upon lightly, because I know that you have had the whole thing taught, and much more efficiently than I can pretend to do it. I refer to *pregnancy*, and I shall only allude to it in its medico-legal aspects. Now pregnancy may be feigned. For instance, it may be feigned for the purpose of extorting money. A woman may go to her paramour and say she is pregnant by him, and so forth. Then pregnancy may be feigned for the purpose of producing a false heir; a woman may pretend to be pregnant if she wants a son, and very important questions in relation to property are connected with her having a son. She pretends to be pregnant, and when the time comes she buys a boy baby, and it gets smuggled into the bed. That has been done, and in recent years.

Then pregnancy may be feigned in order to stay execution. According to the old views, partly theological and partly legal, if a woman was convicted of murder it was lawful to hang the woman, but was not lawful to hang the innocent baby which was possibly in her womb. For this reason pregnancy has been pleaded for a stay of execution, and there have been nice discussions as to when the product of conception has civil rights and obtains a soul. That is a sort of thing we might discuss and not get much further with, and therefore I will not detain you with it. But the law decides that in order that execution may be stayed the woman must be "quick" with child, and accordingly great importance has been attached to the fact of quickening. Of course we know that a foetus is "quick" from the very day of its foundation, and there is no exact time when a foetus quickens, though in the later months of pregnancy the mother may be conscious of the movements of the foetus, and that consciousness of movement goes by the name of quickening. So that when a plea of pregnancy is made, the old form, which is still adhered to, is that a jury of matrons is empanelled, who shall examine the woman who pleads pregnancy, and inform the court whether she is

quick with child. The last case of the kind in this country which I remember was that of Christine Edmonds, the Brighton poisoner. She pleaded she was pregnant, and then a jury of matrons was empanelled, who decided that Miss Edmonds was not quick with child.

Pregnancy, of course, may not only be feigned, but it may be concealed. It may be concealed to avoid disgrace, and it may be concealed by a woman who has the intention of having abortion procured; in addition, it may be concealed by a woman whose intention it is to commit infanticide. Pregnancy is sometimes a very important matter. It sometimes happens that the male owner of an entailed estate dies and leaves no heir, but he leaves a pregnant widow, and there comes the possibility of a posthumous child. The estates do not follow to the next living heir until the result of the widow's pregnancy is known. When there are great estates and great issues hanging upon the results of the pregnancy, of course there is a strong temptation for fraud; and the law of England issues, or used to issue, a writ known as "*de ventre inspiciendo*," of which I have a case here. Sir Francis Willoughby died leaving no son, but a pregnant widow; whereupon the heir prayed for a writ "*de ventre inspiciendo*," the result of which was a declaration on the part of the sheriff and twelve wise women that she was pregnant. Whereupon the court ordered the sheriff "safely to keep her in such a house, and that she was to be viewed daily by some of the ten women named in the writ," etc. Ultimately the Dowager Lady Willoughby was delivered of a daughter.

I do not think I need trouble you with the signs of pregnancy, which are given in your books on midwifery.

With regard to this question of pregnancy, you may have to determine it in the dead as well as in the living; and you must bear in mind the fact which I told you—that the unimpregnated uterus resists decomposition, and that a body is hardly ever too decomposed to enable you to judge as to whether a woman was pregnant. Amongst the signs of pregnancy post mortem is corpus luteum, that is the remains of the Graafian follicle in the ovary. The corpus luteum of pregnancy is very much more visible than the corpus luteum of menstruation which is not followed by pregnancy. The difference between the corpus luteum of pregnancy

1 of menstruation is said to be as follows:—The

Graafian follicle bursts and expels the ovum; and if no pregnancy has followed, then at the end of three weeks the diameter is three quarters of an inch, and there is a central reddish clot and a convoluted wall. If no pregnancy follows the corpus luteum shrivels up and disappears. At the end of a month it is smaller, the clot may be reddish and the wall yellowish; and at the end of two months there is an insignificant scar, and at the end of six months it is absent. But if pregnancy follows, then at the end of a month it has got larger instead of smaller; and at the end of two months is said to be seven eighths of an inch in diameter, with a pale clot, and you can testify to its presence till the end of the pregnancy. The difference is due to the afflux of blood to the sexual organs in the one case and not in the other.

Closely connected with pregnancy is the question of delivery, which is after all the terminal phenomenon of pregnancy, delivery being defined as the expelling of the contents of the womb. It may be pretended, or it may be concealed, just as pregnancy may be pretended or concealed. The signs of delivery are evident first of all in proportion to the near approach of maturity of the foetus; and secondly, in proportion to the interval which has elapsed. If a woman aborts in the early days of pregnancy, it may be difficult to say anything has happened within a little time of the occurrence. But if she goes on to her full term, and the contents of the uterus are expelled, you may testify to the fact of delivery for some time afterwards. Of course there are many questions connected with delivery, and the question of delivery is very closely connected with infanticide. One of the questions which is to be asked is, "Can a woman be delivered unconsciously?" I take it the answer is in the affirmative if there is good evidence that she was unconscious from anæsthetics or opium, or injuries, or anything of that kind. But that is not the question. "Can a woman *otherwise healthy* be delivered unconsciously?" It is certain that a woman may be delivered very suddenly indeed. A woman was admitted into my ward with pneumonia, she having been delivered suddenly and quite unexpectedly the day before, and she stated positively that she had mistaken the on-coming of her pregnancy for the needs of defecation. That is a subjective symptom, and it is a thing which I confess one can hardly understand. One must

have experience to be able to judge of a matter of that kind. All I can say is that a man never mistakes the needs of defecation for the needs of micturition; there is no confusion in that matter. But it is a plea which is sometimes put forward by women, and it is one which is very unlikely to be true, but you will be leaning towards the side of mercy very often if you pay some little attention to it. Babies have often been expelled into privies, and at Barking, at the outfall of the London sewers, they used, some years ago, to have an average of five full-grown children a year. That does not sound very many, but I doubt if they got there unconsciously. On the other hand, what is evident is that with a woman sitting on a privy the uterus may expel its contents very suddenly indeed, but that she should be unconscious of it is quite another matter.

One of the things you have to remember is that delivery may take place post mortem. What I mean is that the uterus may be made to expel its contents by the pressure of the gases of putrefaction. The gases of putrefaction are formed in the abdominal cavity, and exercise a great deal of pressure, and there seem to have been cases in which the uterus has expelled its contents, and has been almost inverted by the pressure of the gases of decomposition.

As to the signs of delivery and recent delivery, that again is an obstetric matter, which I may simply pass over and refer you to your obstetric teaching and experiences.

One of the things you are sometimes asked is whether a woman has at any time of her life been pregnant. I told you yesterday that the presence or absence of a hymen was after all not an absolute proof of copulation one way or the other. And again I would say that the lineæ albicantes on the abdomen, once formed, tend to last for ever, but they may be caused by anything which distends the abdomen.

The next subject with which I have to deal is also one of very great importance, namely, *abortion*. Here, again, the law is very comprehensive. The crime of abortion is defined as "unlawfully administering to any woman, or causing to be taken by her"—and here comes a very important thing in parenthesis—"whether she be with child or not), with intent to procure her miscarriage, any poison or any other noxious thing, or using for the

same purpose any instrument or other means whatsoever; also the use of the same means with same intent by any woman being with child." Now that is very comprehensive, and, of course, the judges, and lawyers, and juries look very much to the question of intent. I wish to warn you, and to say how great is your responsibility when in the practice of the obstetric branch of your profession you find it necessary to bring on premature labour; and I would appeal to you when you find such a procedure is necessary, that unless circumstances should be too strong (and circumstances are sometimes too strong), never to do it without a consultation. If it were said that you had done any unwarrantable act with intent to destroy the contents of the uterus, or to bring about labour, and the death of the woman resulted, you might get into serious trouble. These cases always have been common in every age of the world, and I am sorry to say that they are as common now as ever they were. Listen to what befell a former student of this college only last year. A woman came to him pregnant, and she said she wished to have abortion procured. He told her it was a criminal act, and he would not listen to her at all. One day came a knock at his door, and this woman almost fell into the passage, feeling ill. She was put to bed, and she aborted in his house, and died as a result of an operation performed somewhere else. A friend came and told me about it, and asked me what should be done. I said, "Go instantly and inform the police, and let the man do it himself." His only safety, under circumstances of that kind, would be in absolute openness; there should be no concealment of any kind. That was done, and the whole matter passed off without implicating him one little bit. But it was a very awkward position to be placed in, and one which, if he had faltered for a minute, might have blasted his professional reputation for life.

Another case occurred only last year at the West End. A young doctor had been guilty of an illicit intimacy with a woman, and she suddenly died of peritonitis, and he was accused of having procured abortion, and he was actually committed for trial charged with having done so. But luckily the whole thing was refuted, and there was no evidence of abortion, but there was found an ovarian abscess which had burst, causing the peri-

tonitis. These are good cases, showing how careful you should be in dealing with a pregnant uterus, and how careful you should be in making post-mortems in these matters. It was a very fortunate thing that the post-mortem in the second case fell into the hands of a skilled pathologist.

When abortion has been procured we may have to determine first of all whether or not that which has been expelled is the product of conception or not. Of course there are such things as uterine moles, clots, and so forth, which it might be alleged were the products of conception, but with regard to which you are not likely to come to any misunderstanding if you carefully examine them. Then when abortion has been procured, you have first of all to find out whether it was necessary to procure it or not. If it was proved to be necessary in order to save the woman's life, no criminal indictment would lie. The next point is as to the methods used. The methods used are very often the recognised ones—tents, injections, and so forth, such as are used by the skilled obstetrician for bringing about expulsion of the contents of the womb in order to save a woman's life. But very often sharp instruments are used by those who procure abortion and make it their business, and it is extraordinary to find what coarse and dangerous instruments are used by them. For instance, there is a case on record where a woman was killed by the rib of an umbrella being put up the vagina, which perforated the vagina, and she got septic peritonitis and died. Of course knitting-needles and all sorts of things are used. But there is nothing like a good case.

On Friday, August 6th, 1880, at the Central Criminal Court, before Mr. Justice Hawkins, Mr. and Mrs. Colmer were put upon their trial for the wilful murder of Mary B—. Mrs. B—, aged thirty-six, was a solicitor's widow, keeping a boarding-house, and in March, 1880, she became pregnant by one of her lodgers. Colmer lived at Bristol, keeping an Anglo-American eclectic and progressive medicine warehouse (always be suspicious of these high-sounding names), while Mrs. Colmer lived at Yeovil, describing herself as herbalist and M.D. Pennsylvania. On March 17th the lodger drove Mrs. B— to Yeovil to consult "Madame" Colmer, and drove her back again. On March 19th Mrs. B— went by train to Yeovil, and she was proved to have been some hours at Mrs. Colmer's house; and

Colmer, who had been summoned by telegraph from Bristol, was there also. It was proved that Mrs. B— fainted and lost much blood while in Mrs. Colmer's house. She returned home in a very prostrate condition, and died the next day. The inquest proved that she had been delivered of a three months child, and that a sharp instrument had been used for the purpose. Both prisoners were sentenced to death.

Here, again, I have to remark that by law the crime of procuring abortion is a felony; and if in the act of committing a felony the felon causes the death of another person, that is murder. So the abortifacient, when he fails in his attempt, and causes the death of the woman, commits murder; but the sentence is very rarely carried out in these circumstances.

Mr. Justice Hawkins summed up in this case in a very exhaustive address, in which he read the Act (which I have previously read to you), and said the only question they had to determine was whether these two persons, or either of them, had been shown to be guilty of the offence contemplated by the statute. They were convicted, but the sentence was commuted to one of penal servitude for life, which is very generally thought to be an act of mercy; but whether it is so or not I think is open to debate.

Alleged Hypnotism in the Courts.—A man has recently been convicted of murder in a Nebraska court, the principal witness against him being the wife of one of his victims, in order to be free to marry whom he is said to have killed his wife and the witness' husband. The case will be appealed on the ground that the accused was not permitted to enjoy his constitutional right to be confronted by his accusers, in that the principal witness, when testifying, was not required to face the accused, and that the court erred in permitting the witness to wear a heavy cloak and coloured eyeglasses, purposely preventing the defendant looking her in the eye and thus forcing her to tell the truth. The attorney's defence to this plea will be that the allegations are true, and that while the constitutional right of the accused may have been violated technically, the step was necessary, as three physicians asserted that the woman was under the hypnotic influence of the prisoner to such an extent that she could not testify while facing him. This will afford a grand opportunity for the production of contradictory medical expert testimony.

Medical Record, March 24, 1900.

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A CLINICAL LECTURE

ON

THE TREATMENT OF CANCER OF THE UTERUS.

Delivered at the London Hospital by
Dr. HERMAN.

GENTLEMEN,—My subject to-day is the treatment of uterine cancer. The treatment is of two kinds—palliative and radical. The radical treatment is the removal of the disease, and the palliative is the temporary alleviation of symptoms.

The treatment of uterine cancer, like the treatment of most kinds of cancer, is not very successful; so much so that one able observer, in a paper recently read before the British Medical Association, came to the conclusion that it was doubtful whether the patients were not harmed more than they were benefited by the operative treatment of cancer. By that, I take it, the author did not mean to deny that in many cases life is prolonged and the progress of the disease retarded by operation. But that is only in some cases. There are other cases in which the patients either die from the operation, or the disease quickly recurs; and some think that when the disease recurs after operation it is attended with more pain than is present when it is left alone. This question is difficult to test, and I am not certain that what is stated is the case. I take it that the view of the writer I have quoted is that if you balance the ill results of the operation against the good ones, the two so neutralise one another that in the net result not much good is done. Now I take it that in deciding as to operation we have not to decide that general question; what we have to do is simply to put before the patient what may be expected if operation is done, and also what will happen if an operation is not done, and then the patient must choose. The patient may die from the operation, or she may get some ill consequence, such as a wound of the bladder or ureter; on the other

hand, if the operation is successful, it may be years before the disease returns. There is this that can be said, happily, that statistics from all over the world show that during the last fifteen or twenty years the mortality following operations for cancer has been steadily reduced, and the percentage of cases in which life has been prolonged long after the operation has increased.

The first condition for success in the radical treatment of cancer is that it should be early. In other words, success depends on early diagnosis. The early diagnosis of cancer depends upon the spread of knowledge, both among medical men and the public. A German compiler of statistics has shown the greater frequency of successful treatment among town patients as compared with country patients, the reason being that country patients were beyond the reach of specialists, and sometimes a long distance from any medical man, and therefore their cancers were not early recognised.

I would again remind you that you have to recognise an early case of cancer in most cases by clinical signs, and not by the microscope, the great sign being friability of the growth. There is no other growth on the cervix of the uterus which is friable like cancer; you can dig a piece of cancer out with a blunt spoon. Having recognised the cancer, you have to determine whether it can be removed. There are two ways of determining that: first of all you take hold of the cervix uteri with a forceps, and if the parts adjacent to the uterus are healthy you will be able to pull the cervix uteri down to the vulva. If you cannot do that there is reason to doubt whether the cancer can be removed; but if you cannot pull the cervix down to the vulva, it does not necessarily follow that the cancer is incapable of removal, because it may be that the broad ligaments have been thickened by inflammation, or that there are peritoneal adhesions, but such a case will be more difficult to deal with than a case in which the parts are healthy. The second thing is to examine by the rectum, and feel the condition of the utero-sacral ligaments, because when cancer of the uterus spreads beyond the uterus it invades one or both utero-sacral ligaments. If you can pull the uterus down into the vulva, and the utero-cæcal ligaments are not thickened, the probability is that the cancer can be removed. You may get cases in

which the uterus appears to be movable, and yet when the patient is anæsthetised there is thickening along one utero-sacral ligament so little marked that it is doubtful whether there is thickening at all, and it seems likely that if there be thickening, still the cancer can be removed. The question is, what should be done then? I think in such a case, if you have obtained consent for the operation, it is better to do it and remove the cancer, even should the utero-sacral ligament thickening be due to the extension of cancer, because if there is cancer left behind which continues to grow I think it is not worse than if the uterus had not been removed, whereas if the patient is told she can be cured, and is anæsthetised for the operation, and then wakes up to find that nothing has been done, it will be a shock to her. And it is possible that although you think there is cancer growing along the utero-sacral ligament you may be wrong, and the thickening may be due to old inflammation.

When the uterus is absolutely fixed, and there is no doubt the cancer has extended beyond the uterus, it is no use making any attempt at removal, and none should be undertaken. Of late there have been attempts to embrace even these cases in the field of surgery, but of that I shall speak presently.

Cancer of the vaginal portion is the form which offers the best prospect in treatment. There are two operations which may be done for it—complete hysterectomy, and a partial operation consisting of removal of the cervix uteri. Removal of the cervix uteri is a very old procedure. Marshall, in 1796, removed what was thought cancer from a prolapsed uterus. Cancer was first removed from a uterus in its normal position by Osiander, in 1801. In those days they cut off the cervix, and arrested hæmorrhage by the cautery or by styptics. The operation was not much done, because the results were not particularly good. Later on, in the sixties, the galvano-cautery was introduced. When I was a student the treatment for cancer of the cervix, if it was thought possible to remove it, was to put a loop of the galvano-cautery round the cervix and then burn through it. Where the cancer was limited to the vaginal portion it was completely removed in that way. The objections to that mode of treatment were, in the first place, that often the cancer was so extensive that the loop would not go above the cancer, and that if

the loop was put high up it burnt through the bladder, or through the peritoneum. These possibilities of doing harm, and the frequent failure to get away the whole of the disease, prevented the operation being often employed.

The operation called the supra-vaginal amputation of the cervix was introduced by Schröder, of Berlin, in 1880. The peculiarity of that operation, and the feature in which it differed from what went before it, was that the bladder and uterus were separated from the cervix, and the uterine arteries secured; and this enabled the cervix to be cut off above the internal os. This operation was a great advance.

Complete hysterectomy—extirpation of the whole uterus—was first done for cancer by Freund in 1878. He did it by the abdominal method, and the mortality in the hands of Freund and his followers was so large that it led to the operation being abandoned. Very shortly after Freund's first operation Prof. Czerny advocated vaginal hysterectomy, which at once greatly reduced the mortality; and from that time to this the direction of progress has been towards bettering the method of vaginal hysterectomy. The chief discussion has been as to the relative merits of complete hysterectomy and the removal of the cervix uteri. When the operation was first introduced the results of the partial operation were better in every way. There was a lower mortality, and the proportion of cures was greater. Since that time the mortality from complete hysterectomy has got lower and lower, and the results have been improving. So that while at first the partial operation was by common consent the best operation where it could be done, it is now almost disused, and there are few places in which anything but complete hysterectomy is practised for cancer of the uterus. One of the earliest operations by this method was done by Dr. Blundell, physician to Guy's Hospital. He removed the uterus for cancer in 1828. But I believe the first person to do it was Dr. Langenbeck, of Germany, in 1813. The cases in which cancer of the uterus can be treated by hysterectomy are those in which the disease is limited to the uterus, as shown by the possibility of pulling the uterus down to the vulva and feeling that the broad ligaments are free from infiltration. The advantages claimed for the partial operation, and which really existed at one time, were, first, that there

was less danger. When vaginal hysterectomy was first introduced the employment of antiseptics was not so general as now, and the opening of the peritoneum was more dangerous than it is now. The partial operation had the advantage over complete hysterectomy that it did not entail the opening of the peritoneum. But now, thanks to antisepsis, opening the peritoneum makes little difference to the mortality. It has been claimed also that better results follow the partial operation, and that has been so. But it is difficult to see any reason why, if the cancer is removed, the results should be better if the body of the uterus is left than if it is taken away. The explanation is simple; it is that the partial operation was done in the more favourable cases. In cases in which it was doubtful whether the cancer could be completely removed the whole of the uterus was taken away; and that is why the results of the former proceeding appear better. Another advantage claimed for the partial operation is that after it the patient menstruates, and may become pregnant. This is seldom a real advantage, because the patients who become affected with cancer have generally reached the age when child-bearing is over; and this leaving the stump of the uterus is, to my mind, the great drawback of the partial operation, because there is cicatrization and sometimes atresia, from which the patient may get trouble. I think it is one of the advantages of complete hysterectomy that it does not leave a useless morsel of uterus to trouble the patient. It is said that the disease is more completely taken away by removing the whole uterus. But in the most suitable cases, those of cancer of the vaginal portion, this is not so. You get away no more of the broad ligament when the uterus is removed than when the cervix is removed.

Now I come to describe the operation. First the preparation of the cancer. Of course I assume the operation is preceded by the necessary surgical cleansing by washing with antiseptics. The cancer is prepared by scraping away as much of it as possible with a sharp spoon. This in any case makes the operation easier; because if the cancer is bulky, its mass may hide the parts you are dealing with. It is found that recurrence after operations for uterine cancer generally takes place in the cellular tissue, seldom in the vagina, or in the ovary, or in the lymphatic glands; and it is be-

lieved, and I think correctly, that recurrence often takes place—I do not say always—from the accidental inoculation of particles of cancer in the recent wound. We know that in the abdomen, when a tumour of the ovary is removed, if a bit of it is broken off and left behind, it will become attached to the peritoneum, become vascularised and grow; and I think it is reasonable to believe that cancer of the uterus may do a similar thing.

An able German gynaecologist who recently did us the honour of paying a visit to Victor Ward, told us that in the school from which he came they had discontinued this preparation of the cancer, because they thought that cancer always extended by way of the lymphatics, and that recurrence was not due to accidental inoculation of the wound. I think the reasons for believing in the injurious results of such accidental inoculation are as strong still as they were. Besides the analogy of ovarian tumours showing that a broken-off cancer may take root and grow, this theory of accidental inoculation explains why it is that relapses are less frequent after removal of cancer of the body of the uterus than after removal of cancer of the cervix. It is known that after hysterectomy for cancer of the body of the uterus recurrence is less frequent than after removal of cancer of the cervix, and the explanation is that in cancer of the body there is less chance of inoculation of the wound. In cancer of the cervix it is scarcely possible to remove it without a chance for the cancer to get in contact with the wound. I say this, not that it is certainly the correct explanation, but it is an explanation which is in harmony with the facts. So I think it is a good practice to prepare the cancer by first scraping away as much as you can, and then by cauterising it, so as to cover its surface with a dead black crust.

After that is done the operation may be divided into nine stages. There have been many small variations in the modes of performing this operation, and many of them are of small importance.

(1) The first is to divide the vaginal mucous membrane all round, in front, behind, and at the sides. It is generally stated that this should be done at about four fifths of an inch from the edge of the cancer; but you can hardly lay down an exact measurement for all cases; you must cut round at a sufficient distance to give a margin of healthy tissue; and if the body of the uterus is

large it is well to increase the space you have to deal with. If you have not much space and the uterus is big, enlarge that space by making an incision in an antero-posterior direction in front, and by making lateral incisions at the sides, so as to get plenty of room. It is necessary to divide the vagina all round, because if you leave at the sides a little bit of vagina which will be included in the grasp of the forceps, the forceps will not hold the vessels so well.

(2) Open Douglas's pouch. It does not matter whether you open the anterior or the posterior pouch of peritoneum first; if you open the anterior first, blood trickles down and obscures the view.

(3) Strip off the bladder and ureters. This is best done with the fingers or with a sponge or swab. It is a detail of the greatest importance, because one of the chief dangers in the operation is the danger of wounding or including in forceps or ligatures any part of the bladder or ureters. These accidents have happened to operators of the greatest distinction in all countries. One compiler puts the proportion of injuries to the bladder as high as 10 per cent. In my experience injuries to the bladder are far less frequent. The ureters are more in danger than the bladder. I can only recall one case in which I wounded the bladder, and that was many years ago, when the operation had not often been done. The safety of the ureters is secured by carrying the separation extensively towards the sides. When the bladder and ureters have been well separated you can feel a thin sheet of peritoneum at the top of the front of the uterus.

(4) Secure the uterine arteries. In this step of the operation the greatest divergence arises between the practice of different operators. Some prefer to secure them with ligatures, others with forceps. The forceps method was introduced by Sir Spencer Wells, but it was popularised by the late M. Péan of Paris. Wells secured the uterine vessels with pressure forceps in 1882, and Péan shortly afterwards. I show you Péan's forceps, the drawback to which is that the blades are so long that the pressure at their tips is exerted to some disadvantage, and that they include a great mass of tissue, and this is not all of equal thickness, and thus the vessels in the thin part are not so well compressed as those in the thick part. If you trust to forceps for the arrest of hæmorrhage, it is

of the greatest importance that the forceps should be well made, of good pattern, and strong. Here I show you a forceps which is of a bad pattern, because, however closely you press the handles together you can see daylight between the blades. It is an unsafe forceps. The shorter the blades the greater is the compressive power and the greater the security. Here is Pozzi's forceps, with the blades even shorter than in Wells's. They can be taken to pieces for cleaning. By forceps is one way of securing the uterine arteries, the other way is by ligatures. When you have separated the bladder and ureters, if you put your finger over the broad ligament you will feel the uterine arteries pulsating, and with an aneurysm needle of a rather larger size than is commonly used by surgeons you can pass a ligature over them. It matters not much which of these methods is used. The essential point is that whatever is done should be done well. Sometimes forceps slip, sometimes ligatures slip. If ligatures are not properly tied they will slip.

(5) The next stage in the operation is to open the vesico-uterine pouch; but this may, if you like, be deferred till later. That is done by catching hold of the thin peritoneum with forceps, drawing it down, and snipping through it. Then put in your fingers and tear it open the whole length of the wound.

(6) When the peritoneal pouches have been opened and the uterine artery secured, you free the cervix by cutting through the tissues embraced in the forceps or ligature. I like to be on the safe side, and so use both ligatures and forceps, putting on a ligature first, and then holding the tissue with forceps.

(7) Then I come to a simple process, namely, dividing the anterior wall of the uterus up the middle. This was introduced by Doyen, of Paris, and it wonderfully simplifies the operation of hysterectomy—much more than one could have thought before trying it. When this has been done the body of the uterus can be brought out of the wound without difficulty.

(8) Having cut up the anterior wall of the uterus and got the body outside, the next thing is to clamp or tie the broad ligaments. This done, only one thing remains—

(9) To cut away the uterus.

When you have cut up the anterior wall of the uterus, and your scissors have gone through the

cancerous tissue, you should take another pair of scissors to cut the uterus away. The uterus having been cut away, you next pack a little iodoform gauze into the vagina and put the patient back to bed. If the vessels have been secured there is no after-bleeding.

Now as to the difficulties and the dangers. If the vaginal orifice is narrow, so that you have only a small space to work in, the operation may be rendered difficult. The way to meet that is to incise the vaginal orifice. It is not necessary to make an incision into the ischio-rectal fossa, but a large enough one to enable you to work in.

One danger is the liability to injury of the bladder or ureters. This may be done by including the bladder in the ligatures or in the forceps. It may be interesting to know that in the second case of vaginal hysterectomy on record—by Sauter, in 1822—the bladder was wounded. It was not a suitable case for operation, because there was cancer of the bladder. The patient recovered, but a vesico-vaginal fistula was left. The ureters ought not to be wounded, because the bladder should be separated with the fingers, or with a sponge or swab, and not with such violence as to tear the ureters. The ureters are mostly injured by being included in the ligatures or in the clamp. The only way to avoid this accident is by separating the bladder and ureters widely at the sides. It is said that the rectum has been injured in this operation. The only cases in which the rectum can be in danger are those in which there are adhesions in Douglas's pouch. But in a straightforward case it is difficult to see how, with ordinary care, any injury could be caused to the rectum. Bowel might perhaps be wounded by being included in the clamp. When a forceps is applied with the body of the uterus outside the vulva it is difficult to see how bowel can be included; but when the uterus has been dealt with in the peritoneal cavity, bowel has been caught in the forceps. Still, as the method of dealing with the parts inside the abdomen becomes extinct, this accident is not likely to happen.

But a more important thing in connection with the bowel is that in all intra-peritoneal operations there is risk of ileus occurring. A wound in the peritoneal cavity is closed by the formation of adhesions. After vaginal hysterectomy adhesions are formed in the pelvis, and bowel falls into the pelvis, and gets fixed by adhesions. In time the

adhesions become absorbed, and the bowel gets free again. As a rule peristaltic action is not interfered with, but it is possible that a coil of bowel may be fixed in such a way as to get kinked, and the patient may die of intestinal obstruction. It is infrequent, but there is a risk of it happening.

The great risks are those of septic poisoning and the risk of hæmorrhage. The risk of septic poisoning is set aside by antisepsis. If antisepsis is not used the risk of septic poisoning is great. Risk of hæmorrhage is obviated by care in securing the broad ligaments by forceps or by ligatures.

During the last seven or eight years there have been attempts to apply to cancer of the uterus the principles which surgeons apply to cancer of the breast. When removing the breast for malignant disease they try and clear out the axillary glands as well, and they claim that they get better results the more extensively this is done. So in the case of the uterus attempts have been made to remove not only the uterus, but implicated cellular tissue as well. This cannot be done by the vaginal operation, because in it you cannot see the parts at the brim of the pelvis, and to operate by touch in the midst of such important structures as the ureters and iliac vessels is dangerous. There are two ways in which it has been sought to get at these parts, one the sacral route, by which the uterus is exposed by an incision like that in Kraske's operation for removing the rectum, a piece of the sacrum being removed. In this way it is said the uterus is better exposed to view. The other way is by the abdominal route. Those who have tried both routes say that there is no comparison between them, that the abdominal route is infinitely superior to the other. Therefore, if this more radical treatment should come into vogue, the abdominal route is likely to be the one selected.

Another route is through the perinæum, to get at Douglas's pouch in that way; but when you can get into Douglas's pouch through the vagina it seems superfluous to make an artificial passage through the perinæum, and I see not the advantage of it.

The operation for the extensive removal of cancer is a formidable one. It consists in opening the abdomen; then opening the broad ligaments to expose the ureter; isolating the ureter through its whole course in the pelvis, so as to be sure it shall

not be wounded; then tying the uterine arteries close to their origin from the hypogastric artery; and having thus guarded against hæmorrhage and injury to the ureters, you remove the uterus with a large quantity of cellular tissue at its sides. One Frenchman has gone still further; he ties the iliac artery, cuts through the ureters, and implants them in the rectum, and then removes the uterus and adjacent parts. I do not think that is likely to be generally practised. It is hardly necessary to point out that this extensive operation of dissecting out the ureter through its whole length, and then tying the uterine artery far from the uterus, is a longer operation and a more difficult one than that of simply removing the uterus. It involves more exposure and handling of the peritoneum than hysterectomy. At present the operations which have been performed on these lines are too recent for one to be able to form an opinion on their ulterior results; this can only be done after the patients have been watched for a number of years. The first operation of this kind was done in 1891, so the time is as yet short. My own opinion is that if the uterus is firmly fixed, in all probability the cancer has extended further than the cellular tissue, and it is unlikely that the patient will be cured by removing as much cellular tissue as you can get away by the mode of operation which I have described. One eminent authority has expressed the opinion that if the uterus is fixed, even if it be thought that the infiltration is not that of cancer, it is better that no attempt should be made to remove it, because the fixation will make the operation so difficult and dangerous. It has been said to be an advantage of the partial operation that when the uterus is fixed amputation of the cervix may be carried out in preference to complete hysterectomy. My opinion is that if the uterus is fixed amputation of the cervix is as difficult and dangerous as complete hysterectomy, because the danger of removing a fixed uterus lies in the difficulty of securing the uterine arteries, which will be found lying in densely thickened connective tissue. So I do not take fixation of the uterus as any indication for preferring a partial operation.

The palliative treatment of cancer is practised when the uterus cannot be removed. The symptoms of cancer are pain, hæmorrhage, discharge, and wasting. There is nothing which will prevent

the wasting. The discharge can be lessened by astringents and by deodorising lotions ; but when the discharge is copious and foul there is nothing which will mitigate it or lessen the foulness except great attention to cleanliness. The patient must use a deodorant lotion very often, and have a large supply of clean linen. Nothing but that will lessen the fœtor.

Hæmorrhage varies much in extent ; sometimes it is copious and makes the patient pallid. When the hæmorrhage is great you can check it by scraping and cauterising the growth. If you scrape away all the cancer you can with a sharp spoon, and then cauterise the surface with the actual cautery, hæmorrhage is often checked for a considerable time, and so is the discharge. I have also noticed repeatedly that the pain is relieved by the same process. The reason is that the bleeding takes place from the vessels laid open by the breaking down of the growth at its older and softer part ; and you remove this soft part by the sharp spoon. From the part of the cancer which is beginning to invade healthy tissue bleeding is not so likely to occur.

There are many drugs which can be given for the relief of pain, but only one can be relied upon, and that is morphia. In advanced cases the patient has not long to live, and therefore considerations as to the formation of the morphia habit need not be entertained ; you can give as much morphia as the patient wishes. The most effective, but not the most convenient way is by hypodermic injection.

The best deodorant washes are peroxide of hydrogen and Condy's fluid. When you have relieved the pain and checked the hæmorrhage and discharge, that is about all you can do in the palliative treatment of cancer. Perhaps in the future we may be able to do more.

DISASTROUS mistakes are more easily made in connection with eye diseases than in any other branch of medical science, especially by those in general practice, whose opportunities for seeing eye cases are few and far between. This is a true saying from the preface of 'Golden Rules of Ophthalmic Practice,' the seventh of the series of the clever little publications issued by Wright & Co., of Bristol. The rules form a sound guide upon points on which the ordinary practitioner is most likely to go astray.

A THURSDAY CONSULTATION AT ST. BARTHOLOMEW'S HOSPITAL.

MR. LANGTON showed a child with a tumour in the sacro-coccygeal region. The child was two years of age, and the tumour was present at birth. About a year ago the child was so ill that any thought of operative procedure was quite out of the question. However, now the child had been in hospital for some time, and had improved so much under proper care and nourishment, that the question of operative procedure presented itself. The skin over the tumour was getting somewhat red, and he thought it looked as if it were inclined to break down. Therefore, as was not uncommon in these cases, there was a tendency to subsequent suppuration, perhaps even of gangrene. On passing the finger into the anus, which could be done with comparative ease, the end of the tumour was come upon, and for a short distance the finger passed along the anterior wall of the sacrum in a perfectly natural way. There was, however, some enlargement above it, beyond which the finger could not be passed. The tumour was of the size of a small cocoanut, and there were veins passing over it in front. It seemed to be fairly movable over the sacrum, in the transverse as well as in the vertical direction. As the child was now very much improved in health, he suggested that an operation might be done for the removal of the tumour. No temporising measure could be employed ; the condition must either be left alone or operated upon. He did not think the tumour was growing beyond an extent commensurate with the growth of the child. He need not enter into the various structures of which sacro-coccygeal tumours might be composed ; some were periosteal, some were lymphoid tissue, some were connective tissue, and so forth. He, Mr. Langton, felt very strongly that something ought to be done to remove the growth, especially bearing in mind the great trouble it would be to the child as it grew up. He had put the risks carefully before the parents, and they were perfectly willing that operative measures should be undertaken, seeing what a great burden the tumour would be in later years. Therefore if his col-

leagues agreed with him he would remove the tumour.

Mr. BUTLIN agreed that the time had come when it was advisable to attempt removal of the growth. Mr. Lockwood had helped him remove one a couple of years ago, the patient being a little baby, and by taking up each vessel almost before it was cut the tumour was removed almost without any bleeding. The child made a successful recovery.

Mr. BOWLBY thought that the circumstances were such as to compel operation. A point upon which he laid stress in such cases was the degree to which the tumour extended into the pelvis, and whether it pushed back the coccyx. Some tumours were so far in the pelvis, and pushed the coccyx back so much, that it was impossible to do anything. He did not think this was one of those cases, and he felt that this tumour could be successfully removed. The operation, however, would be of considerable danger to the child's life because of the bulk and vascularity of the growth.

Mr. D'ARCY POWER thought that the tumour could be removed, and that an attempt to do so ought to be made.

Mr. D'ARCY POWER showed a patient, a man about thirty-five, for Mr. Willett. The history was that fourteen years ago he was struck violently on the left side by one of the staves of a barrel which burst. He was able to walk directly after the accident, and had been able to do so ever since. He had had swelling of the lower end of the thigh, and this swelling had increased steadily and caused him some pain and difficulty. Mr. Willett had previously shown the patient at consultations, and it was then suggested that massage should be employed. He had accordingly gone through a thorough course of massage, but there was no alteration in the swelling. Mr. D'Arcy Power exhibited the skiagram which had been taken, and said the patient now came to know whether anything further could be done for him. There was also some disability at the knee, and he (Mr. D'Arcy Power) thought the condition of the knee the more important of the two. As that joint was in a condition of partial ankylosis, he would be inclined at the time of operating on the tumour to at any rate wrench the knee.

Mr. LANGTON thought the patient associated his condition, and his consequent inability to work, to

the two together—the knee and the tumour. He himself was of opinion that the knee condition contributed more to the inability than did the swelling in the thigh. The swelling was probably a so-called exostosis, an osteoma with a tolerably broad base, and he agreed that when operating on it the knee should be wrenched, because he took it that the patient had fibrous union at the knee-joint.

Mr. BUTLIN did not associate the swelling of the femur with the locking of the knee-joint. At a certain stage in flexion the leg came to a dead stop. If the patient were put under an anæsthetic he (Mr. Butlin) would endeavour to break down the adhesions there. Without attention to the knee he did not think the patient would gain much by the removal of the exostosis.

Mr. BOWLBY asked whether the tumour in the man's thigh had anything to do with the fixation of the knee. He did not think it had. There was no interference with the quadriceps extensor, but there was a curious sudden stop at the knee, the exact condition of which was not shown by the skiagram. The question he had in his mind was whether there was any other bony growth at the back of the tibia. He only threw that out as a suggestion for a further examination. Possibly at the lowest part of the femur there might be some growth, because it was well known that osteophytic growths were sometimes multiple. He quite agreed with the suggestions as to treatment.

Mr. LOCKWOOD thought the patient had exostosis on the front of his femur, and it appeared to have been present during most of his life. He did not think that exostosis caused the locking of the knee; he thought he could feel the quadriceps extensor moving over it quite smoothly. Regarding the locking of the knee, he did not believe it was due to adhesion; it occurred suddenly, as if two bony surfaces were coming together. He suggested having another skiagram taken, and if that showed nothing more he would recommend that the patient should be put under an anæsthetic. He thought the knee-joint was the chief source of trouble.

Mr. D'ARCY POWER showed a man aged 28, a policeman, who was formerly a soldier. He came with symptoms which seemed to point to the presence of renal calculus. The history was that in January, 1896, shortly after returning from

seven years' service in India, he had an acute attack in the right loin, with which he was laid up three weeks. The pain was intense, and he had hæmaturia. He recovered from that, but four months later had a recurrence of the same symptoms, and again got well. Two months ago he was again seized with pain in the right loin, passing downwards and forwards, and he had been practically laid up ever since. Tenderness still remained. At times he had passed blood in the urine, and he had had frequent micturition. The point was whether an exploratory operation should not be performed on the case. He understood from Mr. Willett, whose case it was, that he was prepared to cut through the peritoneum to do the anterior operation. His (Mr. D'Arcy Power's) own opinion was that operation was distinctly justifiable. He would recommend that the operator should explore for suspected stone, and should examine both kidneys.

Mr. LANGTON said the symptoms as narrated seemed to be cardinal ones of renal calculus, constant pain, with periodic exacerbations, extending down the thigh from the region of the kidney, with sickness. On the whole he would be inclined to recommend that operation should be performed. He, however, did not think he would go through the peritoneum, but would work from behind. If there were stone, he thought it could not be a very big one.

Mr. BUTLIN thought the symptoms were certainly the cardinal symptoms of stone. On the other hand, he had seen cases in which the same symptoms existed, and in which on exploration no stone could be found. He had seen many cases of suspected stone in his time, and a good many years ago a patient of his died as a result of the exploratory operation. Examination was made, but no stone of the kidney, or ureter, or bladder could be found. The patient had been passing blood, had had violent attacks of renal colic, and the urine contained crystals of oxalate of lime. On the strength of those symptoms he felt considerable confidence in exploring for stone. Dr. Hugh Walsham had skiagraphed his patients, and had discovered stone in the kidneys of two patients. As the kidney in one case was enormously enlarged he, Mr. Butlin, did not think he would have a skiagram taken, although it was a question whether there was tubercle or stone. He discussed the

case with his clinical class, and asked one of the members of the class what he would do in such a case. He answered at once that he would have a skiagraph taken. This was done, and not only one stone, but two stones were discovered. Before the taking of the skiagraph he did not know whether the kidney was tuberculous, or whether it contained stone. In both cases the kidney was low down. One of the difficulties in skiagraphing was where the kidney was high up, and the ribs intervened. In such cases the patient had to take a deep breath and retain it, to push the kidney down as far as possible, so that a skiagraph could be taken successfully. In the present case he thought the best plan would be to explore from the front and feel both kidneys, and if there was a stone, to take it out from the back.

Mr. LOCKWOOD thought the history showed that the man had all the classical signs of stone in the kidney. Two months ago the man was very ill, and there seemed to be fairly good evidence of blood in his urine. The man was still ill and unable to work. Under the circumstances he would recommend extirpation of the kidney. He (Mr. Lockwood) would prefer the lumbar incision, because there was a greater risk of troublesome ventral hernia after the anterior incision. He had seen hernia himself from the lumbar incision, but he thought that happened with extreme rarity.

Mr. WILLETT said his own feeling about the case was that the symptoms of calculus in the right kidney were sufficiently definite and distinct to warrant surgical interference. As nothing could be felt in the shape of a large kidney, even when the patient was under an anæsthetic, and as the seat of pain was associated lower in the direction of the pelvis of the kidney or top of the ureter, and as these were very difficult positions from which to extract a stone, he was rather in favour of a transperitoneal incision, which enabled the surgeon more thoroughly and satisfactorily to examine the region of the pelvis of the kidney. Then he thought it would be possible to make certain of the existence of a stone before proceeding to materially interfere with the relations of the kidney. Of course there was no doubt that the operation from the front was attended with somewhat more risk than the lumbar incision, but on the whole the certainty with which the operation could be performed, and the greater probability there was of

the successful removal of the calculus through the anterior incision, indicated that line of procedure. Therefore, if his colleagues agreed, he would operate by the transperitoneal method.

Mr. WILLETT also showed a small boy, who was likewise admitted with renal colic. He was ten years of age, and the history was that three months ago he suffered acutely from pain in the left loin, with which he was laid up for three days. Two months later he had a slighter but similar attack. Nine days ago he had his third attack. On this occasion he had acute pain in the left loin, which had persisted up to the present time. During the past week he had on several occasions passed blood in his urine. When lying in bed he seemed fairly comfortable. On examination of the abdomen there was still some tenderness in the left loin and over the kidney in front; there was also resistance of the abdominal muscles. He had not had an attack of colic whilst in the hospital. His temperature was normal. On the day after admission the urine was smoky, and contained blood, a considerable amount of mucus, but no pus or crystals. Dr. Walsham had made an effort in the last case and in this to demonstrate by a skiagraph the presence of stone; the attempts, however, had been unsuccessful. In fact, even the ribs appeared the merest shadows, the only distinct object being the vertebral column. The facts, so far as they could be learnt, seemed to point very definitely to some lesion about the left kidney, and he could not help thinking that in all probability there was a calculus in that kidney, especially bearing in mind the nature of the attacks and their sudden onset. The boy was young, and there had not been an opportunity of watching him in an attack. Upon the whole, therefore, he was rather inclined to advocate further delay before operating. The boy did not look healthy; he was pale and rather puffy, and he thought it possible there might be some other explanation for the symptoms than stone,—such, for instance, as tubercle. Quite possibly there was nephritis. He was anxious to learn his colleagues' opinion. If the patient should have a typical attack of renal colic, he thought that an exploratory incision would be warranted with a view of determining the presence or absence of a stone in the left kidney.

Mr. LANGTON thought with Mr. Willett that the symptoms pointed to stone in the kidney. The

boy might have renal disease of some sort after scarlet fever, and he, Mr. Langton, would be loth to do a large operation without further information about his condition. He would let him run about the ward and do work there, to see if he got another attack, and if he did, note the symptoms. With regard to skiagraphy in cases of renal calculi, he confessed he had little faith in it. About a year ago, at a meeting of the Clinical Society, papers were read in connection with stone in the kidney. In some of them instances were quoted in which skiagraphs showed no evidence of stone, and in which operation a few days afterwards revealed a large stone. Therefore distinct absence of evidence in the skiagraph of stone did not at all prove that no stone was present. About two years ago he (Mr. Langton) sent one of his patients to a skiagraphist, who sent back the plate with the remark, "You are quite right, there is stone in the kidney." There was what appeared to be stone in the kidney, and it was in the exact position to be expected. On thinking over the case, he asked the skiagraphist whether the patient had his trousers on. The answer was "Oh yes, but I took great care about that." But it turned out that the shadow was caused by the button on the back of the man's trousers. Therefore the patient was stripped, and another skiagraph taken, and in that nothing whatever was seen. The patient had gone back to India, so he had not seen him again. In the present case he was in favour of a posterior operation.

Mr. BUTLIN agreed generally with Mr. Langton's remarks, but pointed out that skiagraphs were very much improving, and he felt no doubt that by-and-by cases of calculus would be successfully skiagraphed. Of course in both his cases in which the calculus had been successfully skiagraphed the kidney was low down, and that made a good deal of difference.

QUININE HYDROBROMIDE is now issued by Burroughs, Wellcome & Co. in "tabloid" form in two sizes, three and five grains in each. On the occasions when the administration of large doses of quinine are called for, it will be a boon to be able to prescribe quinine hydrobromide, with its reduced liability to cause cinchonism, in a reliable pharmaceutical preparation.

SOME POINTS OF INTEREST IN THE STUDY OF POST-NASAL ADENOIDS.

BY

A. S. COBBLEDICK, M.B., B.S.Lond.,

Late Senior Resident Medical Officer at the Nose, Ear,
and Throat Hospital, Golden Square.

THE many excellent and detailed descriptions of post-nasal adenoids found in all modern surgical and medical works almost render a contribution on the subject superfluous. Most of the text-book descriptions are, however, somewhat brief, and lack detail when discussing the treatment of adenoids, so I have endeavoured to be as lucid as possible in dealing with that important section.

The following remarks are mainly practical, and the result of the study of 1668 cases which I have seen during twelve months' residence at Golden Square; they are drawn from the clinics of the members of the visiting staff, to whom my best thanks are due for kindly allowing me to make use of their material. These cases include individuals ranging in age from infancy to middle life, and are divided into three classes, viz.:

1. Between the age of six months and six years—32 per cent.
2. Between the age of six and sixteen years—54.1 per cent.
3. Over the age of sixteen years—13.9 per cent.

In the first two classes males and females were about equally affected, but in the last group the females affected exceeded the males, the proportion of the two being about three to two. Of the cases occurring below the age of six years only 21 were below the age of three. The youngest infant operated on was six months old. In class three 95 cases out of 232, *i.e.* about 41 per cent., were over twenty years of age, and the oldest operated upon was forty.

The main points to be deduced from these figures are—

1. That the disease is most common between the age of six and sixteen years, *i.e.* it is more a disease of childhood than of infancy or adult life.
2. It is not a common disease in infancy.

3. It is not until after the age of sixteen that the number of females affected exceeds the males.

4. The disease is much more common after puberty than is usually supposed.

5. The disease is a rare one after the age of thirty, but the possibility of its occurrence is indisputable.

Ætiology.—Quite a large proportion of these cases date their symptoms, or an aggravation of previously ill-defined symptoms, from one or other of the specific fevers of childhood, more especially scarlet fever and measles.

There is no doubt that in most of these cases the growths were present previous to the onset of the eruptive fever, and became hypertrophied by the inflammatory processes in the pharynx and naso-pharynx, so common in these diseases.

Hypertrophic rhinitis in varying degree is nearly always present with the adenoid growth; before puberty, however, it is thrown somewhat in the background by the severity of the other symptoms. After the age of puberty the accompanying rhinitis produces most of the troublesome symptoms, and requires more active treatment.

I have never seen post-nasal adenoids associated with well-marked atrophic rhinitis either in children or adults; nor is this very surprising when we consider that one of the leading features of the latter disease is a complete atrophy of all lymphoid tissue in the upper respiratory tract. With early and slight degrees of atrophic rhinitis some adenoid tissue may be met with in the naso-pharynx.

The association of post-nasal adenoids with deformity of the hard and soft palates is not uncommon. The most common type of palatal deformity is the extremely high A-shaped hard palate; another type is the cleft palate, complete or partial.

Cases of cleft palate with post-nasal adenoids excellently serve to demonstrate the position and appearances of the growth.

In children enlarged faucial tonsils are commonly present.

Symptoms.—A well-defined case of post-nasal adenoids presents such marked and well-known features that a detailed account of them is not necessary.

I must say, however, that cases characterised by considerable facial deformity and the train of symptoms termed aprosexia are by no means common.

in this series I have only seen two such cases. The probability is that through early diagnosis and treatment the child is not allowed to drift into this serious condition, a view which tends to support the theory that adenoids are the cause of this condition of aprosexia rather than that the condition is due to mental deficiency from birth associated with the presence of post-nasal adenoids.

The relation of adenoid growth to the highly arched hard palate—that is whether the growth is the result or the cause of the deformity—has given rise to much discussion.

On the one hand the adenoids have been ascribed to the high palate producing nasal obstruction, and so favouring their growth; whilst on the other hand some authorities contend that the growths are the cause of the deformity, through blocking the posterior choana, and so causing its boundaries to fall in by inducing a negative pressure within the nose.

It is almost impossible to definitely settle the question, but it seems most probable that the deformity of the hard palate is congenital, and that the presence of adenoids is merely a coincidence; in short, the two conditions are only associated.

No doubt a negative pressure does exist in the nasal cavity in many of these cases, and is the cause of those well-marked depressions so frequently seen immediately behind the *alæ nasi*; proof of this lies in the fact that early removal of the growths is soon followed by an improvement in the deformity.

One of the most interesting studies associated with post-nasal adenoids is the ear trouble to which they so frequently give rise.

Ear complications are so commonly associated with adenoids in children that any child suffering from deafness, due to either catarrhal or suppurative disease of the middle ear, should at once be suspected as the subject of adenoids.

The types of middle ear disease present may be classified as follows:

- | | | |
|-----------------------------|---|----------------|
| 1. Catarrhal otitis media | { | a. Acute. |
| | | b. Chronic. |
| 2. Suppurative otitis media | { | a. Persistent. |
| | | b. Recurrent. |

Acute catarrh is coincident with the onset of a cold, to which the subjects of adenoids are so

prone. The child complains of earache, is restless at night, and does not take its food well. As a rule the trouble subsides in two or three days.

When the catarrh is more acute suppuration may supervene; the appearance of pus in the meatus is preceded by more severe pain, the tympanic membrane is hyperæmic, and bulges posteriorly.

Rupture of the membrane is followed by free discharge of pus and great relief to the patient. The discharge soon clears up under treatment, and only recurs on the development of another cold. This form of suppurative middle ear disease—*i. e.* recurrent—is always due to the presence of adenoids, and is the most common form of ear trouble which is caused by them.

Chronic catarrh is not of a severe or persistent type, the parents of the child notice increasing deafness, with the ordinary symptoms to which adenoids give rise. Provided the removal of the growth is not too long postponed these cases do very well after operation and a course of politizerization.

The causation of this catarrh is due to the extension of a naso-pharyngeal catarrh to the Eustachian tubes and thence to the tympanum; the Eustachian obstruction which accompanies the catarrh is increased by the amount of adenoid growth around the orifices of the tubes causing some weakness of the levator and tensor palati muscles.

The persistent suppuration of the middle ear, which resists treatment, and causes trouble for years after the removal of any accompanying adenoids, probably always dates from one of the acute specific fevers, especially scarlet fever and measles.

Adenoids are not the cause of this form of ear trouble, but are only associated with it, and this association is not surprising when we consider how often the eruptive fevers of childhood cause an increased hypertrophy of post-nasal adenoids and an aggravation of previously ill-defined symptoms, and at the same time so frequently produce a destructive suppuration in the middle ear.

Two symptoms upon which much stress is not usually laid are—

1. A short, dry, hacking cough; when present it is very typical and almost diagnostic.
2. Enlarged cervical glands; these are small and discrete, and are situated on both sides of the neck,

in front and behind the sterno-mastoid muscles. They do not tend to suppurate, and as a rule disappear after operation if good hygienic treatment is also carried out.

In adults the symptoms are not as marked as in children, and when marked obstruction to nasal breathing is present, it is due to accompanying hypertrophic rhinitis rather than to the presence of the post-nasal adenoids.

There are two distinct types of growth found during adult life, viz. :

1. A localised growth in the middle line near the vault of the naso-pharynx, varying considerably in size, usually of firm consistence and smooth surface.

2. A diffuse, less dense and lobulated growth more or less filling the naso-pharynx, and indistinguishable from adenoid growth found in the naso-pharynx during childhood.

Of the two the former is the much more common condition.

This is a suitable place to note the changes which take place in the naso-pharynx at puberty. At the age of puberty the naso-pharynx becomes more capacious, so that if adenoids were previously present, the relative space which they occupy after this development is lessened ; no doubt this altered relationship has led to the assumption that adenoids always atrophy at and after puberty.

I have no doubt that some atrophy does take place, by reason of the increased nasal breathing space, but it is not as great or universal as is generally supposed. I have now seen so many cases where the post-nasal space has been full of adenoid growth some four or five years after the age of puberty, that I am not at all sure that this atrophic process is so very universal at puberty.

It is difficult to say in which cases the atrophic process is marked, but I should say it is chiefly in those where there is not a great deal of hypertrophic rhinitis, that is to say, where there is not much nasal obstruction.

Diagnosis.—*In children.*—Many cases can, of course, be diagnosed on inspection, but the amount and character of the growth are not always so readily determined.

Generally speaking, if the faucial tonsils and cervical glands are enlarged, if there is bilateral nasal discharge and suppurative otitis media present, a large amount of growth may be diagnosed.

When the tonsils and cervical glands are large, and the only complaint is that the child snores at night, and is at times a little deaf, the naso-pharynx is probably not packed, but adenoids are certain to be present in some amount.

In children under the age of fifteen years nearly every case of suppurative otitis media is, or has been, associated with adenoid growths, so that when this condition is unfortunately present it is almost diagnostic of the presence of some adenoids in the post-nasal space.

These small points are important, as almost any one of them taken together with the subjective symptoms is absolutely diagnostic of some adenoids, and excludes the necessity of a digital examination.

Digital examination is by far the best method of discovering the amount and distribution of adenoids, but a weakly nervous child, who has been once subjected to it, retains a vivid remembrance of it, and is extremely difficult to manipulate at future examinations.

I therefore seldom examine digitally in children, excepting under an anæsthetic preliminary to operation. I have, however, seen three cases in which a wrong diagnosis was made through not making a digital examination. In each case the symptoms were much the same, and were such as to lead to a diagnosis of some adenoid growth, viz.:

Slight deafness due to chronic catarrh.

Snoring at night, and mouth breathing during the day.

Some rhinitis causing a muco-purulent discharge from the nose.

But in none were the faucial tonsils or cervical glands enlarged, and there was no history of suppurative ear trouble.

When examined under an anæsthetic, the post-nasal space was found to be quite clear, but the mucous membrane over the posterior ends of the inferior turbinate bones was enormously hypertrophied and blocked up the posterior choana, so producing marked nasal obstruction. The obstruction was in each case removed with a Mackenzie snare, and the result was as satisfactory as that which usually follows the removal of post-nasal adenoids.

Other elaborate methods of diagnosis seem to me to be unnecessary, such as those which demonstrate whether the naso-pharynx is free or not by

the syringing of fluids up one nostril, or by the use of an oil atomiser, and noting whether they return by the other.

This method only enables one to decide whether post-nasal obstruction is complete or partial, but what is more important it is not a means of differentiating between nasal and post-nasal obstruction.

The only conditions from which post-nasal adenoids have to be distinguished are—

- I. Congenital syphilis with snuffles.
- II. Foreign body in the nose.
- III. Membranous rhinitis.
- IV. Nasal diphtheria.

With ordinary care there is usually but little trouble in making the differential diagnosis.

In adults the diagnosis is usually much more easy on account of the comparative ease with which a rhinoscopic examination can be made. In extremely nervous girls and alcoholics, however, it may be absolutely impossible to get a good view of the naso-pharynx. It is in these unmanageable adults that a digital examination is so useful, because in cases of hypertrophic rhinitis, with a low condition of the general health, the symptoms of post-nasal adenoids are closely simulated; a digital examination not only decides the presence or absence of adenoids, but also the condition of the mucous membrane covering the posterior ends of the inferior turbinate bones.

Treatment. Non-operative methods may be dismissed in a few words. Lane advocates systematic breathing through the nose for short periods several times a day, and states that by this means adenoids will in the course of time disappear.

Why a child should, even for a year, suffer all the symptoms caused by the presence of these growths, to say nothing of the attendant risks, is not clear, when they can be removed by a practically safe operation, which makes the patient very much more comfortable in a few weeks.

Operative measures should be undertaken early, and there is hardly any contra-indication to this rule. Perhaps in a few cases amongst the poorest classes, where the child is in an extremely ill-conditioned state, with a foul suppurative otitis media, operation may be postponed for a few weeks, whilst the ears are well cleansed and the child placed under more healthy surroundings.

Generally speaking, the child and the ear trouble will be much more benefited by the removal of the

growth than by postponing the operation until the child "gets stronger."

Of the removal of adenoid growth without any anæsthetic—quite a common proceeding in Germany and America—I have had no experience, excepting in infants, nor can I conceive that it will ever come into vogue in this country.

It is usual in infants with urgent symptoms to do something without an anæsthetic; enough can be done *pro tem.* by breaking down the growth with the finger-nail, a proceeding, however, which is not always very easily carried out. If desired, they may be removed more completely at this age by means of a small ring curette.

Choice of an anæsthetic.—In this series of cases the choice was made according to age. To those under six years of age, excepting very young infants, chloroform or the A.C.E. mixture was administered, whilst those over six years of age had, for the most part, nitrous oxide gas anæsthesia.

As a rule children do not take gas as well as adults; their tendency is to readily develop cyanosis and opisthotonos, nor is the anæsthesia as prolonged as in adults.

I have notes on three cases, children about the age of six years, to whom nitrous oxide gas was administered in the usual manner by an experienced anæsthetist, all of whom became early asphyxiated, and stopped breathing for an unpleasant length of time, and this before the operation was commenced.

I personally think that all children under fifteen years of age should have chloroform or the A.C.E. mixture administered for this operation, especially if the operator has not had considerable experience in operating on cases under gas anæsthesia.

Of gas and ether for this operation I have no experience, but it is safe and largely in use at some hospitals.

Choice of instruments.—The number of instruments invented and used for the removal of adenoids is probably unequalled by those used in any other single operation. The curettes in common use are—

- i. The original Gottstein.
- ii. Beekman's, which is an extremely good modification of Gottstein's; the window in the instrument is rectangular instead of triangular, and the portion carrying the cutting blade is at right angles to the handle.

iii. Delstanche's and Thomson's modification are both very useful instruments if the operator is desirous of producing the growth, and has not acquired the knack of bringing this out of the mouth by a turn of the wrist; these instruments have a small movable cage fixed by a spring to the front of the cutting blade, and on the cage are two small curved hooks to transfix and hold the growth.

The forceps most commonly in use are Lowenberg's and Jurac's.

Mode of operating under gas anæsthesia.—The patient is seated in an upright position on a high-backed chair, or if possible in an easily adjusted dental chair; children between the ages of six and ten years are, however, very much more easy to manage in the recumbent position.

To control the patient's movements it is advisable to have two wide leather straps passing around the chair, the lower one enclosing the patient's legs just above the ankles, and the upper one passing around the lower part of the trunk. During the production of anæsthesia the mouth is kept open by a dental or a modified Wingrave's gag; if the former is used, a Mason's mouth-gag must be inserted on the removal of the face-piece.

The growth is then removed by a few strokes of the curette; it is not advisable to remove the growth with forceps, unless an unusually tough pad is discovered after the operation has commenced.

The naso-pharynx should be then examined with the right forefinger, and any growth around the Eustachian tubes broken down with the finger-nail. If enlarged tonsils are present there is time to remove these also, the best instruments being Mackenzie's and Heath's guillotine: the latter instrument is a very good one, of simple design, and most easily cleaned; the handle is thick and deeply fluted, and is placed at right angles to the cutting blade.

A practical point of importance with regard to the removal of tonsils is to use the smallest of the three sizes of tonsillotomes; I have not yet seen a case where it was not possible to remove the tonsils with this small-sized instrument. Many large tonsils are really pedunculated, and they should be threaded through the tonsillotome from below upwards, and cut off close to the fauces. Another advantage of the small-sized instrument is

that no damage is done to the pillars of the fauces; if the large-sized instrument is used the operator frequently finds that he only shaves off the surface of the tonsil, and may also cut the pillars of the fauces, and produce persistent hæmorrhage.

Exception to the above remarks should be made with regard to the extremely large unilateral tonsil sometimes found in adults. In these cases a tonsillotome should never be used at all, for it is after removal of these by a cutting operation that many of the worst cases of hæmorrhage have resulted.

It is the best treatment to remove them with a strong snare, such as Lambert Lack's, which is fitted with a strong screw mechanism for gradually drawing the wire sufficiently tight.

In addition to removing tonsils and adenoids under gas, it is frequently possible, if the patient has taken the anæsthetic well, and does not struggle, to snare the hypertrophied posterior ends of the inferior turbinate bones.

The danger of operating with the patient in a sitting posture appears to the spectator to be a real one, but I know of no case of growth becoming impacted in the larynx.

With regard to the removal of post-nasal adenoids under chloroform there are one or two points of importance.

The child should not be deeply anæsthetised when the operation is commenced; the just perceptible presence of the conjunctival reflex is the best guide, so that if blood trickles as far as the larynx a cough immediately gives warning. This comparatively light anæsthesia renders it unnecessary to have the child's head hanging over the end of the table. There is no advantage in the position; it embarrasses respiration, and frequently results in the child suffering for some time from a severe stiff neck. It is better to have the child flat on its back, with one assistant in readiness to turn him bodily on one side, so as to allow the blood to flow from the mouth.

Many operators still prefer to use forceps rather than the curette; if forceps are used, Jurac's will be found much easier to manipulate than Lowenberg's, especially in young children.

Personally I find that almost the whole of the growth can be removed by a few strokes of the curette, and if any growth then remains I finish by picking pieces away with forceps until the naso-pharynx is quite clear. There is a class of case

where the growth cannot be adequately removed with the curette, and forceps must be used; I refer to those cases where the body of the axis vertebra is abnormally prominent, with the result that the adenoid growth lies in a deep concavity between the roof of the naso-pharynx and this prominence. These cases are not very common; only five were met with in this series.

After treatment and results.—After the removal of adenoids no local treatment is necessary; syringing the nose should be avoided on account of the liability to set up acute suppurative otitis media. In many cases the operation is followed by a considerable amount of constitutional disturbance, lasting for two or three days; patients complain of sore throat, pain in the back of the neck and head, and general malaise.

Earache is a not uncommon complaint about forty-eight hours after the operation; it is not a severe form, and is due to a Eustachian catarrh spreading to the tympanum; this subsides spontaneously or after a short course of politizerization.

Acute suppurative otitis media does sometimes follow the removal of adenoid growths; in cases where there has been no previous ear trouble this complication ought not to occur, as it is the result of injury to the orifice of the Eustachian tubes, inflicted at the time of operation; on the other hand, it is not surprising or uncommon to find cases of old suppurative ear disease, which have become quiescent, flare up after the operation as the result of an acute catarrh spreading to the already diseased tympanum.

I have only seen one case of suppurative middle ear disease in which the infective process spread to the mastoid antrum after the operation for removal of adenoids, but I have notes on three cases of mastoiditis in children in whom post-nasal adenoids were overlooked or forgotten at the time that the antrum was opened.

As a rule, the remarkable improvement in the general condition of the child which follows this operation is its most pleasing feature.

There are cases, however, where the usual beneficial result does not accrue, and the operation is only partially successful, *i.e.* the child continues to snore when asleep, and remains a mouth breather by day. This is due to the presence of nasal obstruction, caused either by severe hypertrophic rhinitis, by a marked deviation of the septum, or

by both combined. Hypertrophic rhinitis is the most common cause, and has led some authorities to systematically snare the hypertrophied mucous membrane on the posterior ends of the inferior turbinate at the time that the adenoids are removed.

Deviated septa should not be operated upon until after the age of puberty, otherwise some external nasal deformity may result through lack of growth of the septal cartilage.

It is very necessary to impress on the parents the great importance of educating the child to breathe through the nose, for it is only natural after years of mouth breathing for the child to continue breathing in the same way after the growth has been removed.

It is almost equally important to impress upon them the necessity of regularly administered nutritious food and an abundance of fresh air and exercise, as the general tendency is to feed these children hourly, and expose them to as little fresh air as possible.

It is well to bear in mind that in children the operation is occasionally followed, *i.e.* within thirty-six hours, by an attack of broncho-pneumonia.

THE ANNUAL DINNER of Old Students of King's College, London, will be held at the Holborn Restaurant on June 18th. It is announced that the chair will be taken by the Hon. Sir John Alexander Cockburn, and H.R.H. the Duke of Cambridge, K.G., has signified his intention of being present. Tickets may be obtained from any member of the Committee, or by application to the Hon. Sec. and Treasurer, W. Vincent, Esq., A.K.C., at King's College. The following is a list of the Committee: Professor Henry Robinson, F. & A.K.C., M.I.C.E. (*Chairman*); Sir Hugh R. Beevor, Bart., A.K.C., M.D., F.R.C.P.; Bertram Blount, Esq., F.C.S.; G. Lenthal Cheate, Esq., F.R.C.S.; W. H. Collins, Esq., A.K.C.; Herbert Jackson, Esq., F.C.S.; the Rev. Professor R. J. Knowling, D.D.; the Rev. G. J. Mayhew, A.K.C.; J. C. Melliss, Esq., A.K.C., M.Inst.C.E.; Rev. G. E. Newson, M.A.; James Parkins, Esq., A.K.C.; Rev. Archibald Robertson, M.A., D.D.; A. Clavell Salter, Esq., A.K.C., B.A., LL.B.; Walter Smith, Esq., A.M.I.C.E.; E. F. Wesley, Esq., A.K.C.; John Chapman, Esq., A.K.C. Among the list of stewards are found the following names:—The Right Hon. Lord Lister; Professor L. S. Beale; Professor David Ferrier; Professor Halliburton; Christopher Heath, Esq., F.R.C.S.; Professor William Rose; and Professor Burney Yeo.

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WHAT IS THE EXPLANATION OF THE WASTING OF THE LIMB IN DISEASE OF THE HIP-JOINT?

BY

EDMUND OWEN, F.R.C.S.,

Surgeon to St. Mary's Hospital, and Consulting Surgeon to the Hospital for Sick Children, London.

IF in the course of the examination of a medical student, or of a young practitioner, I ask what is the explanation of the wasting of the limb in tuberculous disease of the hip-joint, he promptly replies, "want of use," and he seems to be perfectly satisfied that his answer leaves nothing to be desired; in fact, he *knows* that what he says is correct. But, as the old doctor remarked in *The Fool's Paradise*, "Knowledge is the monopoly of the young practitioner," and as he grows older it may become forced upon him that there is some other factor in the wasting than mere want of use.

At least, I am by no means satisfied that the theory of want of use fully answers the question with which I preface these remarks. I am quite prepared to admit that when a child is laid up in bed from any cause a certain amount of general muscular atrophy takes place because of want of use. Thus, on the very day on which I write this sentence a boy, from whose right knee I had removed a loose semilunar fibro-cartilage, after having lain ten days in bed, called my attention to the fact that his sound leg was getting small. I dare say that it was. But the wasting to which I refer is something far more noteworthy and important than this—it is an atrophy of bone as well as of muscle affecting the limb with the tuberculous hip-joint.

The term *atrophy* used in its widest sense means want of nourishment, and may thus be applied to a tissue of which the development is arrested as well as to one which, though previously well developed, has now begun to waste. In the case of chronic joint-disease atrophy affects the limb

in both of these senses; muscles which were once properly developed dwindle away to nothing, and the bones involved in the affected joint fail to attain their proper size. The latter of these effects may be best observed in the case of old-standing tuberculous disease of the hip-joint; though the head of the femur remains in its socket, the great trochanter does not project so far as on the other side (allowance being made for the muscular wasting), and it is above its proper level, showing that the neck of the femur is shorter than it should be. And careful measurement reveals the fact that the entire limb has not attained its due length. Moreover, if a shadow-picture be taken the upper end of the affected femur is shown to be more slender than that of the other side. This is not the result merely of want of use, for in the case of a child who has been lying on his back—perhaps on a double Thomas's splint, for instance—for nearly the whole of the time that his hip has been diseased, the affected limb is conspicuously wasted, but the other hardly at all.

I think it fair to assume that in the case of chronic tuberculous disease of the hip-joint the atrophy of the muscles, which is shown by their extensive wasting, and the atrophy of the femur, which is evinced by its failure to attain proper development, are due to the same cause. What is that cause?

In the anterior cornu of the grey crescent of the spinal cord are large multipolar, trophic cells (trophic, τροφικός, nurse; τροφίζω, to nourish) which preside over the nutrition of the muscles, bones, and joints. When, as in anterior poliomyelitis, these cells are seriously disturbed or actually wrecked, they are thrown out of working order, and the tissues depending on them for their supply consequently suffer. And so it comes about that in infantile paralysis not only do the muscles waste, but the bones remain small, and the joints lax.

In infantile paralysis, which is the result of anterior poliomyelitis, the muscles of the affected limb wither, and the bones cease to grow; and all this is undoubtedly the direct result of the storm which has taken place amongst the trophic cells in the front of the grey crescent.

I am inclined to think that the wasting of the limb in hip-joint disease is the result of a similar, though, of course, much slighter disturbance of

the trophic cells in the grey crescent, brought about probably in this manner: When tuberculous disease is started in a boy's hip-joint, the only help that Nature can render is by keeping the limb and the joint at rest, and this she effects by diminishing the irritability and vigour of the muscles through a reduction of their supply of blood under the influence of the trophic cells. In amputating a limb for hopeless tuberculous disease of a joint, has not the surgeon often been surprised at the smallness of the main arteries which he has had to secure?

I feel safe in asserting that in chronic tuberculous disease of the hip the supply of blood to the muscles and, of course, to the bones as well, is diminished.

The office of the trophic cells in the anterior cornu is to govern the nutrition of the limb in all and varying circumstances—in disease as well as in health. To accomplish this there must be harmonious working between the cells and the limb. If the cells are damaged—as in poliomyelitis—the muscles, the bones, and the joint suffer. And if the joint is in trouble (as in tuberculous arthritis) are the trophic cells to remain unimpressed, unsympathetic? I think not. On the contrary, I opine that these cells are at once made aware of the articular inflammation, and that they forthwith set to work to inhibit the wonted arterial flow, in order that the affected parts may be rested to the utmost; and so it comes about that wasting of the limb sets in.

With our present methods of pathological investigation, it may be impossible to demonstrate the existence of molecular disturbance in the anterior cornu of the grey crescent of the lumbar enlargement in the case of a child dying from chronic tuberculous disease of the hip-joint. Possibly it has not hitherto been looked for. But as methods improve, and our powers of observation advance, I expect that such histological changes may become clearly demonstrable. And, if so, the question which heads this short paper will be finally and completely answered.

AN appendix to what is generally known as 'Neale's Digest' has been issued by Messrs. John Bale, Sons & Danielsson, Ltd. The title page of this work is "The Medical Digest, or Busy Practitioner's Vade-Mecum. Appendix, including the years 1891 to March, 1899, by Richard Neale, M.D.Lond."

EPIPHORA: ITS CAUSE AND TREATMENT.*

BY

ERNEST CLARKE, F.R.C.S.

I HAVE selected this subject for this evening's paper for two reasons. Firstly, because its early recognition and appropriate treatment is simple, easily carried out, and saves the patient from much after misery and discomfort. It is the medical man coming most into contact with the patient, who sees the disease in its early stages, and if he knows exactly what to do he can, in most cases, speedily effect a cure. And secondly, because opinions are so constantly varying as to the best method of treating the various forms of the disease, that I felt it might serve a good purpose to inquire into this subject and try to arrive at a correct opinion.

I must first briefly refresh your memory concerning the anatomy of the parts under discussion.

The lachrymal gland consists of two portions—an upper, or main, portion, and a lower, or accessory, portion,—and it lies under the roof of the orbit on the outer side and towards the front, in contact with the conjunctival fornix under the upper lid, where its ducts (about twelve in number) open.

The tears pass over the eye and find an exit by means of the two puncta lachrymalia situate at the inner part of the margin of the upper and lower lids. These puncta are the external openings of the canaliculi, which are two small canals leading to the tear sac. The tear sac is a space lined with mucous membrane situate behind the inner palpebral tendon, blind above, but opening below, and continued as the nasal duct to the inferior meatus of the nose, where it terminates. The nasal duct is about three quarters of an inch in length.

The way the tears pass into the canaliculi is partly by the act of winking, which, by putting the internal palpebral tendon on the stretch, performs a sort of suction action, and partly by capillary attraction. The tears pass from the sac by the action of gravity. It is most important to remember this, for when once the canaliculus has been slit up in all its length capillary attraction is

entirely done away with, and the suction-pump action is lessened and often permanently destroyed, so that we have to depend on gravity to prevent epiphora.*

Epiphora is a running over of the tears on to the cheek, as distinguished from lachrymation, which only signifies an excessive secretion of tears. The causes of epiphora may be divided into two classes:

- I. Where the tear passages are normal, and
 - II. Where the tear passages are abnormal.
- I. Epiphora with normal tear passages:
 - (a) Emotional display.
 - (b) Irritation of the fifth nerve:
 1. By foreign bodies in the cornea or under the lids.
 2. By irritating gases such as ammonia, or the vapour from onions, or by cold wind.
 3. By excessive light.
 4. By an error of refraction.
 5. By various diseases of the eye. For instance, corneal ulcer is a very common cause. Gouty infarction of the Meibomian glands may, by producing conjunctivitis, cause epiphora.

In all these cases epiphora results because the passages are too small to carry off the "storm water."

II. Epiphora with abnormal tear passages; divided into two classes:

1. Malposition of the puncta lachrymalia with or without some obstruction.
2. Puncta normal, but obstruction in some part of the passage.
 1. Malposition of the puncta lachrymalia. Normally the lower punctum (and it is with this lower punctum we are most concerned) looks backwards and very slightly upwards and touches the eye, but the slightest eversion of the lower lid drags the punctum away from the eye, and it will be found to be looking upwards, and sometimes even forwards, and under these circumstances it is incapable of performing its duty and epiphora results.

The causes of this eversion, which displaces the punctum, are many. Among the most common are—

Chronic inflammation of the lid and conjunctiva,

* There are two other factors that, according to Hasner and Cahn, probably assist in causing the downward flow of the tears; the elasticity of the sac and ducts, and the aspiration of the tears from the nasal duct into the nose during inspiration.

* Delivered before the New Cross Medical Society.

granular lids, and blepharitis; the lid becomes thickened, the punctum is drawn away from the eye, epiphora results; this causes irritation of the skin, which leads to contraction, causing more eversion, and in a very short time, through this vicious circle of events, a well-marked ectropion exists. Facial palsy, wounds of the lower lid or parts of the face contiguous, and burns, may each cause eversion, and consequent malposition. Styes and small sebaceous tumours situate on the lower lid close to the punctum easily displace it. And so we see that anything which causes eversion or ectropion will be almost sure to cause epiphora.

The ectropion in the aged, which is due to a laxness of the skin and tissues, is a very common cause of epiphora. We all know the senile, glassy-looking eye, with a tear shining in the corner.

Inversion of the lid may also be a cause of epiphora. The irritation of the lashes causes lachrymation, and even if the puncta are not displaced they may be unable to carry off the excessive flow. But very often the puncta are displaced; they point downwards, and are seen to be removed from the eye.

Epiphora from spasm of the orbicularis, producing inversion, is a frequent result of bandaging the eyes, and unfortunately often complicates the after-treatment of a cataract or other operation.

2. Obstruction in some part of the tear passages. This obstruction may be—

(a) In the punctum.

There may be congenital absence of the puncta; but it is very rare. A foreign body, such as a lash, or a bristle, or the awn of a piece of barley, may be sticking in the punctum, or long-standing blepharitis may have closed the orifice. All these causes are fairly rare, and it is very unlikely to find atresia of the punctum, even when it has not been used for many years, as in long-standing eversion of the lid.

(b) In the canaliculus.

A small polypus may be growing from the mucous membrane of the duct, or it may be blocked by a tear-stone—a dacryolith.

But, with the above rare exceptions, the seat of obstruction is in—

(c) The tear sac and nasal duct.

Obstruction in the nasal duct is the prime cause, and this leads to retention or partial retention of the tears; and then follow inflammation of the duct and of the sac, blennorrhœa, dacryocystitis, etc.

There seems to be no doubt that in most cases this trouble *originates in the nose and naso-pharynx*.

The nasal duct opens into the inferior meatus of the nose just below the inferior turbinated bone, and the opening has a slit-like valve; the mucous membrane of the nose is in direct connection with the mucous membrane of the duct; and what wonder then that inflammation in the nose easily spreads to the duct. This inflammation of the mucous membrane of the duct may be very slight, and in no sense acute; but it causes swelling, and the narrow passage becomes still narrower, and retention of tears, which may only be partial, may at any time become complete through an accession of the inflammation.

Snuff-takers often suffer from a chronic form of rhinitis, which sometimes leads to nasal duct obstruction.

Thickening of the bony canal may result from periostitis in syphilitic subjects, or from periostitis caused by a carious canine tooth. An inspection of the skull will show how near the long root of a canine tooth is to the nasal canal.

It is very rare that the tear passages are infected from above. Look at the hundreds of cases of gonorrhœal ophthalmia and ophthalmia neonatorum that never lead to inflammation of the tear sac or duct; and yet, if the trouble could start from the eye, it would surely be these cases that would do it. The reason, I believe, is that the tears sweep the infectious secretion quickly away, and the healthy mucous membrane is only exposed to it for a very short time.

Gummatous growths and polypi may block the sac and duct, and cause obstruction.

Dacryocystitis, or inflammation of the tear sac.—Primary inflammation of the sac is rare; it has been known to occur in strumous children, or it may be produced by external violence, or by an irritant fluid entering the sac. It has also been known to result directly from a carious tooth. As a rule, inflammation of the sac is a mild chronic affection at first, and is caused by obstruction in the nasal duct or spreading of inflammation from the duct.

We may have at first a *mucocœle*, the sac is distended, not inflamed, and on pressure viscid clear fluid or simply tears pass out at the puncta. This has also been called *dacryocystitis catarrhalis*. If this condition persists the contents of the sac

become turbid, and we get dacryocystitis blennorrhoea.

The chronic distension of the sac may at any time develop into the acute condition and acute dacryocystitis with suppurative inflammation. The lids and neighbouring parts become thickened, brawny, and often acutely inflamed, resembling erysipelas, and there is more or less constitutional disturbance. Very often the swelling is not over the sac, but a little lower and further from the nose; but on pressing the side of the nose close to the inner canthus great pain is felt, and the spot is hard and brawny. If left to itself the abscess points about the situation of the bony margin of the orbit, bursts, and a lachrymal fistula is formed. It has been known to burrow and open close to the ala nasi, or into the antrum or back of the nose.

Occasionally the abscess empties itself through the punctum and the sac does not perforate, but this is rare.

When the fistula has existed for long with discharge going on all the time, extensive sinuses may be formed, and caries of the underlying lachrymal bone may result.

The pus in lachrymal abscess is generally thick and creamy, and is not necessarily offensive in odour. It is sometimes green in colour.

Treatment.—I would lay down as a golden rule, never use the knife or probe if you can cure the epiphora without.

Ascertain first whether the epiphora is constant or only occasional; this is most important, for if occasional it probably points to some outside cause, and the passages may be normal.

The lachrymation caused by an error of refraction will occasionally lead to epiphora. I have treated a large number of these cases, and by prescribing glasses have cured them. The commonest error that induces this is hypermetropic astigmatism, and it is generally of a low degree, and only correctly estimated under a mydriatic.

Therefore, if on inspection you see nothing wrong with the puncta, can make out no rhinitis, and especially if the epiphora is not constant, have the patient's sight carefully tested, and give the glasses, if ordered, a fair trial. If the epiphora is constant, the puncta in their natural position, and you strongly suspect some obstruction, the next step is to syringe the canal. Dilate the punctum

carefully with the dilator, turning it into the horizontal position as soon as you have entered the orifice, then introduce the fine nozzle of Anel's lachrymal syringe, having previously filled it with a warm weak carbolic solution or perchloride of mercury lotion $\frac{1}{3000}$; use only mild pressure, and watch the result. The fluid may regurgitate through the other canaliculus, pointing to obstruction at the junction of the canaliculi and the sac; it may distend the sac, pointing to obstruction in the nasal duct; or it may pass out freely into the nose, causing the patient a disagreeable sensation at the back of the pharynx. This, of course, shows there is no obstruction and perhaps only a little catarrh, and a few syringings will effect a cure. If there is not a free exit, but not complete obstruction, do not resort to the knife at once, but try the effect of syringing every day, using a mild antiseptic solution or a weak alum lotion, and in cases where the obstruction has been caused by a slight localised swelling of the mucous membrane a cure will result. If the obstruction appears to be at the junction of the canaliculus and sac, slit up the canaliculus according to the well-known method, which I need not detain you by describing, and at the same time nick the stricture in the same way that you nick a hernial band, then try the syringe again, turning it into the vertical position when in the sac, and if the fluid passes freely into the nose there is no need to probe.

I am fully convinced that more harm than good is frequently done by probing; the probe is pushed down very often with considerable force along the duct, and the periosteum and mucous membrane which line the bony canal are scraped off. But especially would I protest in the strongest manner against the enormous probes advocated by Dr. Theobald and others.

Theobald gives the following dimensions of nasal duct and probe for comparison:

- Bowman's probe, No. 6—1.5 mm.
- Theobald's probe, No. 16—4 mm.
- Average size of ten adult nasal ducts—4.47 mm.
- Largest size of ten adult nasal ducts—5.15 mm.
- Largest of seventy bony nasal ducts—7 mm.

These may be the sizes of the bony canal, but this size is considerably reduced by the periosteum and thick mucous membrane which line the canal, and moreover, it is impossible to ascertain beforehand what the size is, for it has no relation to the size of the face, etc.; consequently, if we attempt to pass Theobald's large probe we may be dealing with an abnormally small duct, and, of course, with disastrous results, as far as that duct is concerned.

If there is eversion of the punctum proceed as above, and certainly do not probe if there is *no* obstruction. In those cases it is advisable when you have slit up the canaliculus to snip off a small portion of the ocular side in order to make a good "gutter," and also prevent the cut surfaces from reuniting.

We now come to cases where there is complete obstruction in the nasal duct.

First of all thoroughly examine the nose, and see if the obstruction is there. You may discover abnormal position of the inferior turbinated body, or of the septum, rhinoliths, well-marked rhinitis with great swelling of the mucous membrane blocking the inferior opening of the nasal duct, or cicatricial stricture caused by ulcers.

If treatment of these conditions fails, or if they are not present, then we must resort to probing.

Having proceeded as before (for although some surgeons attempt to probe without first slitting the canaliculus, it is not to be recommended) we take a medium-sized probe—not too small, and certainly not too large—I think the small end of Weber's probe is a very good standard. Pass the probe along the slit canaliculus into the sac, and then turn it vertical, passing it downwards slightly backwards and outwards. *Do not use undue pressure.* The first part of the duct is very narrow, and you will often feel the probe slip through it with a jerk, and then it passes on to the orifice, which is again narrow, and where very probably the obstruction is. If the patient blows his nose the presence of blood (from the cut canaliculus) on the handkerchief will show you that you have entered the nose. Probing successfully is a much more difficult operation than it sounds, and false passages are very easily made. (The commonest mistake that beginners make is to turn the probe into the vertical position *before* it has reached the sac.)

The above operation can be done under cocaine, but in nervous subjects it is better to use an anæ-

thetic. If there is distinct obstruction to the passage of the probe do not use undue force, but pass down a Stilling's knife (with a blunt point), and cut through the stricture; or use a smaller probe, and try to gradually dilate the stricture.

Having passed the probe, on withdrawing it pass in a style, pushing it down so that the head lies in the "gutter" formed by the two sides of the slit canaliculus.

The best form of style is the "nail-head" silver style. Leave the style in for, say a fortnight, then remove it and clean it, syringe out the duct with some mild antiseptic solution, and reintroduce the style. It can be worn for some months without inconvenience, and before finally discarding it, it should be left out for a few days only, and then replaced. Some intelligent patients learn to remove it in the morning, and replace it again on going to bed.

In cases where the patient has a great dislike to wearing the style, the probe must be passed at frequent intervals, at first every third day, and after a fortnight once a week for about a couple of months.

In some cases complete bony obstruction is present, and a passage must then be drilled into the nose, and a style left in.

In lachrymal abscess in young children foment and poultice, and if pointing open with a small knife, making your incisions as near the sac as possible, and passing downwards and slightly outwards.

If the fistulous opening does not close, or the inflammation subside, both of which generally happen, slit up the canaliculus, and leave in a style.

In old patients it is generally advisable to slit up the canaliculus at the time of opening the abscess, as the nasal duct will have to be probed before a cure results. If the patient is seen early enough, slitting up the canaliculus will often do away with the necessity of opening the abscess through the skin.

When the discharge is very offensive leave in the duct an iodoform or boracic style.

I may mention that it is useless to try and cure or alleviate a bad ectropion, where, of course, the punctum is everted, without first slitting up the canaliculus, and making sure that you have no lachrymal obstruction, and if you have, passing in a probe or style.

In old lachrymal fistula put in a style, and close the fistulous opening by cauterisation, or by paring the edges and strapping over the wound.

When nasal trouble is present I most strongly recommend the application of the principle advocated by Arbuthnot Lane,* of increasing the respiratory capacity, and consequently the vitality and resisting power of the individual, which, of course, at the same time exercises a most beneficial local effect.

* 'Edin. Med. Journ.,' vol. vi, p. 209.

THE first volume of a Manual of Medicine edited by Dr. Allchin has now been published by Messrs. Macmillan & Co. It deals with general diseases, diseases excited by atmospheric influences, and the infections. An unstinted measure of praise must be awarded for the two plates appearing in the first pages of the book; it can be said without hesitation that nothing better has been done in a work of this description. The editor, Dr. Allchin, has well described the scope of the manual in his able introduction, where he says that the old-fashioned separation of diseases into "general and local" has taken too firm a hold to be lightly discarded in a student's manual. Opinion is not yet prepared for a plan of dealing with the subject which would abolish the time-honoured category of diseases of the various systems of organs, and substitute a grouping under their causal conditions or manifestations of general pathological states rather than under the organs, which in many cases are not always mainly or primarily affected. In the work will be found an account of the various forms of disease, more especially from the point of view of their clinical manifestations and treatment. The subjects of ætiology and morbid anatomy are briefly summarised. The aim has been to present such a picture of the several maladies as will conform to the appearances detected at the bedside, and enable the observer rationally to administer such treatment as the art of medicine affords. The editor must be congratulated on the list of contributors he is able to place before the medical public, and if a slight grumble may be permitted when discussing such a valuable book, it is that the portion actually written by the editor himself might undoubtedly have been greater.

WITH DR. LEWIS JONES IN THE ELECTRICAL DEPARTMENT OF ST. BARTHOLOMEW'S HOSPITAL.

Neuritis from Injury.

I HAVE mentioned to you on previous occasions that neuritis caused by various injuries accounts for a very large number of our patients. In fact, we constantly have in attendance here patients suffering from the effects of injury to some of the nerves of the upper limb. In the lower limbs the cases of injury to nerve-trunks are much less common. They include cases of neuritis from dislocations of the shoulder, and from other injuries in that region, neuritis from pressure, such as the pressure of crutches, of splints and improperly applied bandages, and that form of neuritis from pressure which is commonly called sleep paralysis. There are also a few cases where the pressure which has caused neuritis has been due to the occupation of the patient. A third class is due to direct injury of nerves. These may be clean cuts of the nerve, as in many accidents with cutting instruments or broken glass, or may be lacerations or contusions; they may be associated with fracture of bones which have wounded the nerve at the moment of the fracture or have involved the nerve in the callus in the process of healing.

Let us consider these classes a little more in detail.

In dislocations of the shoulder there are several forms of paralysis which may occur. Sometimes the dislocation chiefly damages the upper cords of the brachial plexus. In others the stress of the damage seems to fall more upon the lower part of the brachial plexus, while in the worst cases there may be complete paralysis of all the nerves of the upper limb. Horsley has shown that injury to the upper cords of the brachial plexus is especially produced when a patient falls heavily on the point of the shoulder. A blow in that position tends to force apart the head and the shoulder, and to overstretch those cords of the brachial plexus which lie most directly in the line of the limb,

namely, the upper ones; and in dislocations the same strain upon the upper cords may be produced. The lower cords of the brachial plexus in their passage into the arm take a rather more arched course, and therefore permit a certain amount of movement at the shoulder before the strain actually comes upon them. But after the displacement of the humerus in dislocations fresh troubles may arise, because the head of the bone in its new position is likely to press upon nerves. Thus one may say that in such an accident as dislocation of the shoulder there is a special danger to the upper cords of the brachial plexus at the moment of the blow, or of the violence which causes the dislocation; and there is a second special danger to the lower cords of the plexus afterwards from the pressure of the displaced head of the humerus. And it is especially in cases of dislocation which have been left some time before reduction that the effects of pressure on the lower portion of the brachial plexus become manifest, showing themselves by paralysis of the muscles supplied by the ulnar, or median, or musculospiral nerves, as you have seen in three cases who have just been before you.

The characteristic paralysis which follows injury to the upper cords of the brachial plexus is known by the name of Erb's paralysis, and it has many points of clinical interest, and is by no means uncommonly met with in practice. In this department one or more may generally be found under treatment. The name of Erb's paralysis is given to a combined paralysis of the deltoid, biceps, brachialis anticus, and the supinator longus and brevis. Occasionally the spinati or the rhomboids may also be involved. Erb's paralysis is not due to any special disease, and it may be produced by various diverse causes. It had been recognised as a clinical entity before the time of Erb by Duchenne, but it was Erb who first explained why such an apparently peculiar combination of muscles should be simultaneously paralysed. And he pointed out that an injury in the neck damaging the fifth and sixth cervical roots on their way to the plexus, or damaging the short conjoint trunk which they form *en route*, will cause just that distribution of paralysis which now bears his name. There is a motor point in the neck from which the associated muscles of Erb's group can be thrown into simultaneous action. It is situated a little way external

to the outer border of the sterno-mastoid, at the junction of its middle and lower thirds. An injury which causes Erb's paralysis is not always limited exactly to that point, and so you will meet with cases of paralysis *plus* something else; for instance, it is not unusual to find a patient having a paralysis of Erb's distribution, with perhaps an extension into some of the extensors of the wrist. Some little time ago we had here an interesting case of Erb's paralysis where the injury had involved parts lying a little higher than the fifth and sixth roots. It had therefore injured the phrenic nerves, which, as you know, are given off from the fourth cervical nerve-root. It was the case of a boy who was looking down into the well of a lift, when the lift came down on the back of his head, and compressed his neck against the barrier over which he was looking. He fortunately escaped with his life, but the pressure produced a double paralysis of his phrenic nerves and a double Erb's paralysis. He gradually recovered from both.

There are three clinical groups of cases in which Erb's paralysis is met with: viz. in infants where traction on the arm during delivery has injured the nerve-roots; secondly, in cases of new growths pressing upon the nerve-roots near their points of exit from the spinal canal; and thirdly, from direct violence, as in falls on the shoulder and dislocations of the shoulder. I am glad to be able to show you a case of this kind, because it is important that you should be familiar with Erb's paralysis, and know what to look for when you suspect its existence. I also show you the effect (on the sound side) of electrically stimulating Erb's motor point. The injury in this case was a fall into the hold of a ship, the combined trunk of the fifth and sixth was torn across. It has been reunited, and the patient is now recovering under electrical treatment. Between the time of the operation for suturing the nerve and his return for treatment in this department he has been absent, untreated, for more than six months, and in all that time he made no progress towards the recovery of his lost power, but very soon after the beginning of his electrical applications he commenced to recover voluntary power, and he is now at work.

From time to time you will meet with cases of very severe injury to the shoulder, in which there is a peculiar condition of the muscles lasting for a

week or more after the injury. In these worst cases you will find the muscles of the arm hard, inelastic, and feeling almost wooden, with total loss of sensation, and total loss of all electrical reactions, even from the first. The prognosis in these cases is bad. The hard condition of the muscles probably signifies an œdema of the substance of the muscle, and a total cutting off of the nerve-supply. Occasionally some reaction of degeneration may become visible in the later stages of such a case; some few muscles may be less severely damaged than the rest, and may recover a little voluntary power; sensation may also return in part. Otherwise it is safe to prognosticate in these cases of wooden hardness in the muscles that the limb is irreparably damaged. It has been suggested that some of these cases have suffered a tearing out of the nerve-roots from the spinal cord, and in some of them this view is supported by evidence of some injury to the cervical sympathetic, with contraction in the pupil on the injured side. You will remember the cervical sympathetic arises in the upper part of the thoracic region, including the first dorsal nerve, and might easily be involved in such an accident as the tearing out of the first dorsal nerve-root from the cord itself.

Of the various paralytic conditions which may follow injuries in the region of the shoulder perhaps the commonest is an injury to the circumflex nerve, with a paralysis of the deltoid, and we have them here in abundance. The deltoid has a very important function to fulfil; from its situation it is rather exposed to injury, and sometimes shows a certain tendency to recover imperfectly or slowly when damaged. It is, therefore, wise to give a somewhat guarded prognosis in cases of injury to the circumflex with paralysis of the deltoid, and especially in elderly people. Otherwise the course of such cases is fairly favourable, as in most cases of neuritis from injury. Paralysis of the deltoid is easy to recognise by inability to raise the arm, and on inspection and comparison with the sound side the flattening of the shoulder which results is easy to recognise. Often you will find some impairment of mobility in the shoulder-joint in cases where the deltoid is paralysed. This may be due to the original injury having damaged the interior of the joint, or it may be secondary to the injury of the circumflex nerve which supplies the joint as

well as the muscle. It is best not to begin forcible passive movements to break down adhesions in the joint until the voluntary power in the muscle has begun to return. Another form of paralysis caused by injuries near the shoulder is the paralysis of the serratus magnus. The nerve to the serratus magnus runs through the body of the scalenus medius in the first part of its course, and may possibly be pinched by sudden forcible contraction of that muscle, and it may also be injured by pressure upon the shoulder. For example, there was a patient here with paralysis of the serratus magnus which had been caused through pushing upwards with his shoulder against an iron bar, which he was using as a lever to move a heavy weight. Another patient was engaged in a warehouse in the City, and carried heavy bales of goods upon one shoulder. He also strained his shoulder by overhand bowling, and from one or other of these causes he developed a weakness of the arm and shoulder that he could not account for, but which proved to be due to paralysis of the serratus magnus. Paralysis of this muscle causes a characteristic deformity which is easy to recognise when you have once seen it. If such a patient is told to extend his two arms horizontally in front of him while you look at his back, the scapula will become prominent, and project like a small wing on the paralysed side. The nerve to the serratus magnus and the muscle itself are both rather awkwardly placed for electrical testing and treatment. Perhaps the best method is to have the active electrode on the serrations of the muscle at the side of the chest, and the other above the clavicle and outside the sterno-mastoid. The current under these conditions will pass down the nerve and into the muscle, and movement may be seen in its serrations, and if it is in good order its contractions will be felt to pull the scapula forwards if a hand is placed on the shoulder-blade. These cases do well provided the nerve is not absolutely cut across in the injury.

If we now leave the shoulder and turn to the arm and forearm, you will find that in these parts injuries to the nerve-trunks are very common. We must consider the following groups of cases, for in all of them electrical testing and electrical treatment are of great importance: (1) Paralysis in cases of fracture; (2) Pressure paralysis; (3) Contusions; (4) Incised wounds dividing the nerves.

In cases of fracture complicated by paralytic symptoms you must inquire closely as to the time when the signs of nerve injury first showed themselves, in order to learn whether the injury to the nerve was caused by the violence which produced the fracture or not. In the first case you will have to determine whether the nerve is lacerated at the seat of fracture (as by the broken ends of the bone), or whether the violence may have broken the bone at one place, and hurt the nerve at another. In the second case the paralytic symptoms may first appear some days after the accident, and may be due to pressure from bandages or splints, or they may appear later when the bone is re-uniting, and be the result of the implication of the nerve in the bony callus thrown out.

There is a patient attending here at present for an affection of the musculo-spiral of this kind. He fractured his humerus in April. He developed paralytic symptoms in June, and he was operated on in August; some callus was chiselled away, and when the wound was healed he was referred to this department for treatment. His present condition is one of gradual improvement. The wrist drop, which was his chief symptom, is nearly gone, and now (January, 1900) he has nearly recovered voluntary power, although his electrical reactions are still very bad.

Pressure paralysis.—Injury to nerves from the pressure of splints or bandages is not so uncommon as you may think. At the present time there are two patients attending the department with serious paralysis of the nerves of the hand and forearm from this cause. It is only just to say that the mischief was done elsewhere, and not at this hospital.

A little time ago I saw a patient who had been stabbed, and had received wounds of both his median and ulnar nerves. When he came for testing he showed in addition a partial paralysis of his musculo-spiral from a tight bandage. This form of paralysis from pressure is most frequently seen among patients in whom the bandaging has been applied by an unskilled person, and not a medical man, particularly when the bandaging has intentionally been made tight to arrest hæmorrhage. I remember a case where the patient, a Canadian, had an accident with a revolver when far from help in the backwoods. The brachial artery was wounded, and bled freely. He bandaged up the injured part (it was at the bend of

the elbow) as tightly as he could, and rode back for help. When he reached medical assistance three days later his median nerve was found to be paralysed from the pressure.

Other common forms of paralysis from pressure are crutch palsy and sleep palsy. Both affect the musculo-spiral nerve chiefly, and produce an extensor paralysis which you easily recognise by the "wrist-drop" which follows.

Sleep palsy is a paralysis due to pressure of the head upon the arm during heavy sleep while sitting at a table; or the pressure may be caused by leaning upon some hard substance, such as the back of a chair or the edge of the stairs, during sound sleep. It rarely follows sleep in bed, or ordinary sleep of any kind, and among hospital patients it commonly occurs on Saturday nights. It is the heavy sleep of alcoholic intoxication which leads to sleep palsy by rendering the subject unconscious of those early symptoms of pain and numbness which would probably awaken a lighter sleeper. And I will here say that those who take alcohol freely are specially liable to traumatic neuritis, the alcohol predisposing to neuritis very strongly; I have often noticed that. For example, a potman broke his leg, and after using crutches for a few days only he developed a musculo-spiral paralysis from the pressure of the crutch upon the armpit of the affected side. A year before he had had a sleep palsy of the other arm.

Another group of cases are those where the nerve has been divided by a knife, or by splinters of glass. These are especially common in the forearm; perhaps the ulnar and the median nerves suffer the most often. The important point in these cases is to be able to answer the question whether the nerve is cut across, or whether it is only injured without any solution of continuity. If it has been cut across it must be joined again before electrical treatment is attempted. It is useless to expect recovery by electrical or any other treatment in the case of a nerve which has been severed unless the ends remain in good apposition, which is the rare exception. The part which electricity has to play is first to indicate the place at which the severance has taken place, and later to receive the patient from the surgeon saying whether union has been accomplished as the result of his efforts, and to promote the subsequent recovery by electrical treatment.

When you are called upon to report on the condition of a nerve which has been divided, and subsequently joined by a surgical operation, you will find it difficult perhaps to answer the questions which are put to you. For instance, the muscles belonging to a divided nerve show a reaction of degeneration after the nerve has been sutured, and will continue to show that same reaction of degeneration for some considerable period afterwards. The longer the time which has elapsed between the dividing of the nerve and the reunion of it, the longer will be the period of reaction of degeneration in the muscles. How, then, can you say from electrical testing whether the nerve-trunk is joined successfully or not? You can watch the condition of the reaction of degeneration, and if it remains good in strength and easily elicited, you may say that the condition of the muscle is not failing, and you may infer from that the union has been successful. If, on the other hand, the reaction of degeneration becomes feebler and harder to provoke, you may say that the nutrition of the muscle is failing, and you may infer that the union of the nerve has been unsuccessful. But even in successful cases the reactions may continue to grow feebler and feebler, although everything is going on well. In such a case you will derive considerable assistance from a careful testing of the sensation in the affected area, for sensation returns more quickly by far than the power of voluntary movement, and a return of sensation will be the first sign that things are going on satisfactorily. In testing sensation you must remember that you are more or less at the mercy of the patient, and take precautions to prevent misleading statements. You will also be guided in your report by the appearance of the skin, and by the bulk of the muscles of the affected area, and by signs of return of a little voluntary power. If the wasting of the muscles is arrested it is well; if the skin acquires a more healthy appearance than before the operation, and if sensation returns even in part it is well. In any case you must not be in too great a hurry to say that the operation has failed, for it may be months before you can get good electrical proofs to the contrary. Even in a perfectly straightforward case of division of the external popliteal nerve, sutured immediately, and healing by first intention, I have known a hundred days to elapse before the return of any voluntary power in the affected muscles.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXIII.

GENTLEMEN,—Now there comes the question of birth in its relation to legitimacy, and I would remind you that birth is a very important fact. Perhaps that is a truism, but it is legally a very important fact, even though a being born does not survive. And whether a child has been born or not often influences the disposition of property, as I showed you by means of the diagram which I put upon the blackboard in illustration of what is known as *possessio fratris*. Again, there is what is called tenancy by the courtesy of the law in England. Tenancy by courtesy, as I understand it, is this: that if a man married an heiress—that is to say a woman who has property entailed on a male,—and if she have a son, then her husband enjoys the property for life, even though the wife and son both die. But since the passing of the Married Woman's Property Act a tenancy by the courtesy of the law has become almost impossible.

Now what is birth? In order that birth may be established in the eyes of the law a child must be entirely born, and there are cases where that has been argued. Entirely born means that the child shall be entirely free from the body of its mother—that is to say the whole of the body shall be out of the vagina. It does not seem to be necessary that the umbilical cord shall be cut or that the placenta shall have come away. When a child is born you must, of course, always notice the exact time; and in cases of twins that is a very important matter. If twins be born they do not both inherit; the first born gets the property *in tail male*. You all know the Biblical case, where one put out its arm and a red string was tied round it, and then it withdrew the arm again, and the brother came first. In the case of twins it is very important you should be able to say which of these was born first.

The next point is that the child must not only be born, but it must be born alive. And in some of these cases there have been great disputes as to whether a child was born alive or not. The question then arises, What are the *criteria* of live birth? I need hardly remind you that if a child enjoys

property *in tail male*, and lives only for a minute, the fact that it lived is of very great importance.

Now there are two *criteria* of life ; one is movement and the other is crying. With regard to movement, in 1806 there was the case of "*Fish v. Palmer*," which was tried by the Court of Exchequer. It appears that an infant was born to Mr. Fish in 1796, which was supposed to be stillborn, and on the death of his wife he resigned her property to the legal heir. Some circumstances occurred afterwards which induced him to bring an action and to attempt to prove that the child had not been born dead. Dr. Lyon had declared an hour before birth that the child was alive ; and, having directed a warm bath to be prepared, gave the child to the nurse to be immersed in the warm water. It did not cry nor move, nor did it show any symptoms of life ; but while in the water, according to the testimony of two females—the nurse and the cook—there twice appeared twitchings and tremulousness of the lips. Dr. Lyon then directed them to blow into its throat, but it never exhibited any other sign of life. Several physicians were examined as to the deductions to be drawn from these symptoms. Ultimately it was decided that the child was born alive. That is an interesting case, and it was practically decided on the testimony of the nurse and cook that they saw twitching. The Scotch law demands as proof of life that the baby shall cry. Of course, if a child is born and cries lustily the matter is settled ; but disputes do not arise in plain cases of that sort, but only in those instances such as I have read to you, where there is a great deal of property and very little life. I have said that the Scotch law demands crying as evidence of life ; but in the first place the child may be born dumb. It may therefore move without crying. The next point is that the child may cry before it is born, and there have been several instances on record in which a child has cried in the vagina ; and it is said there are one or two cases on record in which a child has cried *in utero*—that is to say, having got its mouth opposite the orifice, it has made some sort of noise ;—so that you may get crying without birth, and birth without crying. It seems almost stupid to take up your time with all these little points ; and although there is a maxim, *De minimis non curat lex*, still you will find, when property is in the balance, that the law does care about trifles, and will play with them by the hour together.

In France there is not only the question of live birth, but there is the question of vitality—that is to say, whether the creature born is capable of going on living. If the creature is so deformed that it cannot go on living, then the French do not recognise it as a real child. According to Coke, a monster cannot inherit ; "a being which hath not the shape of mankind cannot be heir to or inherit land, although brought forth within marriage"—that is Coke's dictum. Still a dictum of that kind does not help us very much, because where does the shape of mankind end or begin ?

Now the next point one has to decide in reference to birth is, is the child legitimate or illegitimate ? That is a very important matter. According to the law of England, any child born in wedlock is legitimate ; that is to say, if people are married at two o'clock, and a child is born at three o'clock—to take a very extreme case,—that child is legitimate provided the people married come within the necessary limits of affinity as set forth by law. By the law of England a child born out of wedlock cannot be legitimatised ; but according to the law of Scotland the child can be legitimatised by the subsequent marriage of its parents.

Now, seeing that a child born in wedlock is considered as legitimate, it follows necessarily that the legitimacy of a child born in wedlock can only be upset by the force of bad moral circumstances ; and whenever the legitimacy of a child has been upset it has always been as the result of bad moral circumstances.

The legitimacy of a child has been brought in question in a notable case which occurred in Scotland, where a minister of the Scotch Church married, and a child was born at a time after wedlock which caused some scandal to the elders of the Church in which he officiated and to the congregation ; and then the legitimacy had to be gone into. The congregation wanted to expel him and to hold him up to obloquy. Finally it was settled that, although the child was born at the end of the sixth month, the evidence went to show that it was a premature child, and there was no real evidence to show that the child had not been procreated in wedlock.

With regard to the question of moral circumstances and legitimacy, one of the most noted cases is the Gardner Peerage case. But before going to that I will just remind you as to the period of

gestation. The ordinary period of gestation is two hundred and eighty days, or nine calendar months, or ten lunar months, or forty weeks. But pregnancies may be protracted; and there are cases on record where a pregnancy has been protracted to the 300th, 308th, 315th, and 322nd day.

The next point is as to how soon a child may be born and yet live. I would remind you that questions of legitimacy have arisen when a child has been born in wedlock and there has been a non-accessibility on the part of the husband to the wife—*i. e.* where for some reason they have been living apart, and have not been accessible to each other. But unless there is strong proof to the contrary, if a man be, as the old phrase says, "within the four seas of the realm," then his accessibility to his wife is taken for granted. Of course, if a man is in an asylum or a hospital—and still more, if he is in prison and under lock and key—the law would admit that there was non-accessibility.

The Gardner Peerage case is one which has occupied a good deal of attention. It came before the House of Lords in 1825. Allen Legge Gardner, the son of Lord Gardner by his second wife, petitioned to have his name inscribed as a Peer on the Parliament roll. The Peerage was, however, claimed by another person, Henry Fenton Jadis, who alleged that he was the son of Lord Gardner by his first and subsequently divorced wife. It was contended that the latter was illegitimate, and in order to establish this point the evidence adduced was partly medical and partly moral. Lady Gardner, the mother of the alleged legitimate child, parted from her husband on board of his ship on the 30th January, 1802. Lord Gardner went to the West Indies, and did not again see his wife until the 11th July following. The child, whose legitimacy was disputed, was born on the 8th December of that year. Therefore the plain medical question was whether a child born forty-four weeks and four days after intercourse (from January to December), or twenty-one weeks and three days (from July to December), could be considered to be the child of Lord Gardner. If this were answered in the affirmative, then it follows that this must have been a premature or a very protracted birth. There was no pretence that this was a premature case, the child having been mature when born. The

question then was reduced to this, Was this alleged protracted gestation consistent with medical experience? Many medical witnesses, comprising the principal obstetric practitioners in the kingdom, were examined on this point. Their evidence was very conflicting, but the large majority concurred in the opinion that natural gestation might be protracted to a period which would cover the birth of the alleged illegitimate child. On the moral side of the question, it was clearly proved that Lady Gardner, after the departure of her husband, was living in open adulterous intercourse with a Mr. Jadis, and on this ground Lord Gardner obtained a divorce from her after his return. He subsequently married a second wife, by whom he had the claimant, Allen Legge Gardner. It was contended that the other claimant was really the son of Lady Gardner by Mr. Jadis. The decision of the House was that this claimant was illegitimate, and that the title should descend to the son of the second Lady Gardner.

Therefore you see that in spite of the medical evidence as to the possibility of such a protracted conception, the decision was one of common sense, and one with which we must all agree. The decision appears to have been based chiefly upon moral circumstances, for had not the first Lady Gardner been living in open adulterous intercourse at the time of her husband's departure, it is highly probable, from the medical evidence bearing that way, that the legitimacy of the child would have been allowed. I remind you again that a large amount of property was hanging in the balance, and the point was one worth fighting for, and I daresay any amount of fees were paid to the lawyers, and to the doctors.

The next point is as to what degree of immaturity is compatible with life. Of this there are two cases on record, the Scotch case of which I told you, in which a child was born 174 days after marriage, and it was shown that the child was exceedingly immature, and that it wanted a great deal of care taken of it. This was heard not in a court of law, but in a moral court, and the elders decided that there was no proof that the child had not been procreated within the period of wedlock. There was a foreign case where a child born at the twenty-fifth week lived.

Now we go to another subject which is of very great importance, and that is the question of

infanticide, and the question of concealment of birth. I would remind you that not only is infanticide a crime, but concealment of birth is likewise a crime, by 24th and 25th Vict., Cap. 100, Section 60:—"If any woman shall be delivered of child, every person who shall by any secret deposition of the dead body of the said child, whether that child died before, at, or after its birth, endeavour to conceal the birth thereof, shall be guilty of misdemeanour, and, being convicted thereof, shall be liable, at the discretion of the court, to be imprisoned for a period not exceeding two years, with or without hard labour." Infanticide, that is to say, the killing of a newly-born child, is murder, and these cases usually excite great sympathy, so that those who have been convicted of infanticide are practically never hanged. In almost every case the children who are the victims are illegitimate. A girl has been seduced and a child is born; the birth of the child very likely means the mother's sure ruin, and she tries to conceal the birth and to kill the child. It is not at all surprising under these circumstances that there should be a great deal of sympathy excited. Now in order that the crime of infanticide may be established, you must have certain evidence that the child has lived, and we have to answer certain questions. First of all, a dead child being found, we may be asked whose child is it? or is the dead child the child of the person charged? And therefore we have to say whether the prisoner has been recently delivered. Again, we have to decide in reference to the child; is it mature? was it born alive or dead? and if born alive, what was the cause of death?

As to question one, this will have been fully discussed before you in your obstetric lectures. As to the maturity, that was alluded to when we were discussing age.

Now we come to the question, Was the child born alive or dead? That is a very important matter, and you will find in works on medical jurisprudence that there is a great deal of hair-splitting, if I may say so, on that question. What are the signs of live birth?

The great sign of live birth is the fact that the child has breathed, and if the lungs have been inflated there is no difficulty at all in the matter. Lungs which have been inflated are perfectly different in appearance from those which have not

been inflated. The lung which has never been inflated looks more like liver than lung, and when the thorax is opened such lungs are found lying near the posterior wall of the thorax. Moreover, a non-inflated lung does not crepitate, it is dark in colour, and sinks in water. The lung which has been inflated floats in water, it crepitates freely, and if the child has given some really lusty cries you will have no difficulty. On the other hand, if you find that a very few air-cells have been inflated, what have you to say? I advise you to be exceedingly cautious in what you say under these circumstances. You can only judge by appearance, and using a lens you may see a few air-cells have been inflated on the surface, which cause a characteristic mottling. But do not make too much of trifles. It does not follow because a sickly child has given a gasp, and has got a little air into its lungs, that it has lived in the proper sense of the word. Taking a merciful view of the case, it may be a question of feebleness of respiration on the part of the child which has led to its death. Do not therefore take extreme views. In works on medical jurisprudence you will find tables given upon which you are asked to come to definite conclusions, such as by the weight of the lungs in proportion to the weight of the body. It is said that a lung inflated during life, in consequence of the afflux of air and blood, weighs more than before inspiration has taken place. It necessarily does, but I do not think these proportionate numbers are absolutely certain, or, to put it plainly, I do not think they are sufficient to hang a woman upon, and after all that is the point. You must always have in mind the fact that children differ enormously, and that a child at full term may weigh only two or three pounds, or it may weigh as much as twelve or fourteen pounds. Then again, just as some people inherit big noses or big hands, others may inherit big lungs. Therefore I say I think these proportionate figures are not good enough, and I do not advise you to use any of the hair-splitting arguments of that kind in a coroner's court.

Another point which you must remember is that if a child has lived some time you will be able to come to a conclusion by examining the fetal vessels. For instance, you will be able to say whether the foramen ovale is closed, whether the ductus arteriosus is closed, and again you will be

able to form a conclusion sometimes by the state of the umbilical cord. If you find that the umbilical cord shows any signs of inflammation around the root of it, or, still more, cicatrisation, then, of course, that would be proof positive that the child was born alive.

Another point which is sometimes dwelt upon is the expulsion of meconium by the child from its bowel. I do not think you must take that as a sign of life. Of course it may be so, but it is perfectly clear that if a flabby child is manipulated the meconium may be squeezed out; you may squeeze out the meconium from a child just as you may squeeze the juice from a lemon.

A point to which I attach greater importance is the presence of food in the stomach. It is not conceivable that a child could have milk in the stomach, or any other form of food, unless it had been able to suck.

Then, again, you must remember, in regard to the state of the lungs, they may be fully inflated, and still the child may not have been born alive; but in that case, of course, there would be evidence forthcoming that artificial respiration had been resorted to. When a child is stillborn, one of the first things we have to do is to perform artificial respiration, and it must be performed for some time. The emptiness of the bladder is also of some use as a sign of live birth.

If the child is dead, how did it die? A child may die from neglect—that is to say, by the omission of certain things which were necessary—or by active interference—that is to say, by acts of commission. Of course children have been killed by accidentally getting the cord round their necks, by hæmorrhage from the cord, and so forth; and you would, of course, look to see whether that had happened. Children may also be killed in other ways; and I would remind you that such a thing has been known as putting a knitting-needle through the fontanelle. So that in making a post-mortem you would examine the child's body very carefully to see if there were any marks made by the insertion of sharp instruments into the belly or the fontanelles. Then it is alleged that children have been suddenly expelled from the uterus and killed in that way. It is stated that a pregnant woman has been mounting a pair of steps when the pains of labour came on, and the child was shot out on to the floor and killed. Well, you of

course want evidence of it. We must admit the possibility; women do go up ladders, and they do have full uteri, and they do have sudden births, and it is really a matter of evidence in any particular case, and I do not think it need be very seriously discussed medically. If there is evidence on such a point you ought to receive the evidence with the greatest possible caution, for your common sense will tell you such a thing is very unlikely.

A form of killing children is smothering, and that is a crime which is very common in London; that is called death from overlying. That generally occurs with very young children, sometimes newly-born infants; and it occurs most often between Friday night and Monday night, as shown very conclusively by the Registrar General's returns. I take it that drink is answerable for a good deal of the overlying in London. The child gets between its drunken parents, and they lie and roll upon it; and sometimes, I have no doubt, intentionally. When you are asked to make a post-mortem of a case of death from overlying you must see whether there is evidence of death from asphyxia. I have no doubt that infanticide in that way is much too common.

DR. ALFRED HILLIER, believing that a concise manual dealing with all the hydra heads of tuberculosis in one volume would form a work of reference of some interest and value to practitioners of medicine and medical students, has duly written such a work, entitled 'Tuberculosis: its Nature, Prevention, and Treatment.' The author has been fortunate in his publisher and printer, Messrs. Cassell & Co., and the value of the book is enhanced by thirty-one illustrations and three coloured plates. The author hopes that the chapters dealing with prevention in everyday life, as well as by legislation and public action, may have some interest for members of local bodies controlling sanitation and public hygiene, and in this all will concur. It is at least reasonable to expect that Dr. Hillier's advocacy of the more extended use of the tuberculin test in cattle will receive some attention at the hands of agriculturists. Tuberculosis is a social as well as a medical problem, and we wish the book the great success it deserves.

DYSPEPSIA.

Dr. NESTOR TIRARD.

"A FREQUENT cause of dyspepsia lies in some error of diet, which may affect either the quantity, or the nature, or the frequency of the meals. With regard to the quantity of food in general, undoubtedly many people eat too much for their requirements, while leading the comparatively sedentary life enforced by civilisation. Alteration in the habits of the individual with regard to exercise may suffice to enable him to continue his ordinary routine of diet, but when the conditions of life prevent the devotion of much time to physical exercise, it is in general safe to advise some diminution in the quantity of food. With regard to the nature of the food, one of the most frequent causes of dyspepsia is the consumption of too large a supply of saccharine or of fatty matter. Very often, however, the error consists in the too frequent consumption of food, the individual overloading the stomach before the whole of the previous meal has been thoroughly digested. On the other hand, I have known many instances where the trouble resulted from an attempt to live on two meals a day, the patient taking an enormous breakfast before going to the City in the morning, and endeavouring to abstain from food of any kind until dinner in the evening. This attempt led in one instance to such heavy nitrogenous meals that albuminuria resulted, and was readily cured when once the cause of this symptom had been recognised.

"Dyspepsia will often ensue when liquids are taken in either too great quantity, or at too high a temperature, or of an unduly irritating nature. It is, I think, scarcely sufficiently recognised that, so far as the digestive system is concerned, almost as much harm may result from the consumption of inordinate quantities of strong tea, as from the consumption of inordinate quantities of alcohol.

"It is commonly thought that the harm which often follows the consumption of tea is due to the presence of an excess of tannic acid, and that if the tea is not allowed to stand too long this source of evil can be avoided. Although strong tea will undoubtedly disorder digestion, and will also influence the circulation prejudicially, these evils may follow when weak tea or coffee is taken at a

high temperature. Those who desire the stimulant effect of tea, and accordingly drink it as hot as possible, are very prone to dyspeptic trouble. In the former case, the tannic acid undoubtedly disorders the digestive functions by reducing the amount of gastric juice secreted, and by favouring the coagulation of food; but when tea is too hot it serves as a direct irritant to the wall of the stomach, and causes over-secretion of mucus. Mere bulk of liquid will also give rise to dyspepsia, since the fluid not only serves to dilute the gastric juice, but, by the rapid distension of the stomach, appears also to inhibit its formation.

"I have known a case of severe dyspepsia result from the foolish habit of emptying the coffee-pot at breakfast. The patient, an extremely abstemious young man, exhibited symptoms which might readily have been mistaken for the results of chronic alcoholism. The nausea, furred tongue, and morning sickness readily disappeared when he gave up the absurd practice of over-distending his stomach at one meal. Careful inquiry showed that he was not taking too great a bulk of liquid in the twenty-four hours, but that the error consisted merely in over-distension of the stomach at breakfast time."

The quotation "of making many books there is no end" is not generally used in a complimentary sense, but Dr. Tirard's text-book on 'Medical Treatment,' just published by Messrs. Churchill, impels one to think that if all works were as his it would be to the general good that there should be a continuance of such literature. The passage taken from his work which precedes this paragraph will enable the reader to judge for himself of the practical utility of Dr. Tirard's teaching. The book will need no advertisement to command a wide distribution, for it is of that rare class of publication which "goes off by itself."

Hypertrophy of Recti in Pregnancy.—

Durante shows that although the recti abdominis atrophy when displaced by abdominal tumours, the number of fibres actually increases during pregnancy, so that, although stretched and flattened, the muscles are really hypertrophied.

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A CLINICAL LECTURE ON ASTHMA.

Delivered at the Hospital for Consumption and Diseases
of the Chest, Brompton, November 15th, 1899,

By F. J. WETHERED, M.D., F.R.C.P.

LADIES AND GENTLEMEN,—I take it that those who do us the honour of coming to our lectures at this hospital would rather wish that the lecturer should give some of his experience in the treatment of the disease under consideration than that he should adopt the usual plan of giving a learned discourse on the pathology or the morbid anatomy of the disease; and also that those who are here would prefer to see and examine cases than listen to the voice of the lecturer. But in speaking of asthma there are two or three objections to this plan. I have some cases waiting who are the subjects of asthma, but as a rule there is very little abnormal to hear in examining the chest, and therefore I should like to say a little about the clinical aspects of the condition. I shall, however, lay the greatest stress on the treatment of asthma and the methods which are generally adopted, more particularly a plan of treatment which is only adopted at two places in this country, namely, at Ben Rydding and at this hospital. Of that treatment I have had a good deal of experience, and I believe it is one of the most valuable methods of treating asthma which we possess.

In delivering a clinical lecture on asthma it is important at the outset that a clear understanding should be arrived at as to what the lecturer really means by this term. Unfortunately the word asthma, qualified by a descriptive adjective, is still commonly applied to a variety of conditions which, although differing in origin, present a likeness to one another, owing to the fact that all are accompanied by attacks of difficulty of breathing, cough, and expectoration. This has naturally given rise to some amount of confusion, which is certainly to be regretted. By asthma, however, this afternoon I

wish to indicate simply paroxysmal or intermittent dyspnoea, which is usually accompanied by cough, bronchial secretion, and a feeling of constriction and suffocation, which attacks, of course, are very familiar to all of us. Now this definition includes two forms of asthma. We are obliged to take these two forms because they are so closely allied; and although we do get typical examples of one or the other, yet the greater number of cases are intermediate between the two. These two forms are true spasmodic asthma, in which a nervous factor is the most marked feature of the disease, other changes being in some cases entirely absent, or at any rate in all of them secondary. The other form of asthma is bronchial asthma—a variety of the disease in which paroxysmal attacks of dyspnoea of the true asthmatic type occur in an individual previously the subject of bronchitis and emphysema. The definition which I give here does not include other forms of dyspnoea, sometimes described under the term asthma, namely, dyspnoea accompanied with heart disease, sometimes unfortunately called cardiac asthma; nor that accompanying renal disease, and called renal asthma; and I shall say but very few words indeed about “hay asthma.”

The symptoms of an attack of spasmodic or bronchial asthma I need not describe; we are all perfectly familiar with them. But a few words may be said in reference to the physical signs during the spasm, because this helps us in some degree to understand the pathology of the disease. When a patient is suffering from an attack of asthma the difficulty lies not in inspiration, but in expiration. The former—namely, inspiration—is short and jerky, but the expiration extremely prolonged; in fact the chest is fixed in a position of extreme inspiratory distension. On examination the percussion note is found to be hyper-resonant, the areas of cardiac and hepatic dulness being very much lessened, or perhaps altogether absent. On auscultation the chief feature is an almost complete absence of respiratory murmur. The whole chest, indeed, seems to make very violent excursions or plunges, but there is no corresponding inspiratory wave when listened to with the stethoscope. Adventitious sounds—rhonchi of all sorts—are usually numerous, but the peculiar feature about them is that they are extremely variable. There are sibilant rhonchi, and so on, and they may be extremely

loud; but suddenly they all disappear, and the breath-sounds become again clear. This appearance and disappearance of rhonchi is extremely characteristic. In addition there is another sound which is extremely apt to be mistaken for some sound in the lung. I have mentioned it before in speaking of pulmonary tuberculosis—I mean fascial creaking.

Those are the signs during a paroxysm of asthma. Between the paroxysms the physical signs vary with the variety of asthma. If the variety be truly spasmodic the breath-sounds in the intervals of the attack are quite normal. These forms of truly spasmodic asthma we do not often get at this hospital, but we do get the bronchial forms very frequently. For instance, among a number of patients whom I have ready for your examination to-day there is only one instance of the spasmodic form. The reason, I am afraid, is that the patients have been to all the hospitals and under various doctors, and obtained but very little relief from their sufferings because they are hospital patients. I lay stress upon that because I shall have to refer to it again when speaking of treatment. I shall then tell you that the treatment of these cases of spasmodic asthma is mainly climatic. These patients live in a particular neighbourhood because they are obliged to; they cannot take advantage of a *change* of residence, and, as I shall mention, I am afraid drug-treatment is of little value in such instances.

If we listen to the chest between the attacks of bronchial asthma we hear the usual signs of bronchitis, generally accompanied by those of emphysema, and they last in a greater or less degree of intensity for long periods of time—days or weeks, and adventitious sounds are very numerous—sonorous and sibilant rhonchi are heard over both sides of the chest, and not fleeting as in true attacks of spasmodic asthma. In typical cases of this kind there exists what I am accustomed to call the “complaining chest.” All sorts of loud, rough sounds are heard, and you cannot help thinking when listening to such a chest that it is groaning, and straining, and protesting loudly against the difficulty which it has in moving. As the attack ends expectoration generally begins. First of all, in true cases of spasmodic asthma the expectoration takes the form of small pellets of mucus, something like tapioca, as Salter

described them. After these are thrown up the sputum becomes more and more copious, then it is frothy and thin, and after the patient has been able to expectorate considerable quantities the attack soon subsides. On looking at this sputum it is not very characteristic, nor is it so under the microscope in a case of truly bronchial asthma. But in the spasmodic form are found some very characteristic ingredients. In specimens of sputa under the microscope there will be found a large number of various forms of epithelium—pavement epithelium is very common, and epithelium with cilia upon it, showing that the cells have been torn off from the mucous membrane, and have not lain for any appreciable time in the lung. We also find what are known as Curschmann's spirals, and they are very pretty objects. The first thing we see with a half-inch lens is a very bright, highly refracting spiral, which appears to shine almost like silver. Surrounding it, in the typical form, is a mesh formed chiefly of epithelial tissue. These cells probably come from the very smallest bronchioles. They are not characteristic of asthma, although they are found most often in that disease. I have found them, though only rarely, in cases of pulmonary tuberculosis in which no asthma was traceable. That is the typical form. Occasionally we only find a central thread, and at other times we find the sheath without any central thread. Another very characteristic condition, which we get in the sputum is the presence of certain crystals known as Charcot-Leyden crystals. They are sharp-pointed, and sometimes exist in very large numbers indeed. I have sometimes found them crowding the field of observation. These crystals do sometimes occur in other conditions, but both the crystals and the spirals are more common in asthma than in any other disease I know of.

Now, as Dr. Goodhart points out, the clinical history of spasmodic asthma is by no means completed by the ordinary dyspnoic attack. There are several other irregular states that are no less part of the disease, though they are not generally recognised as such. There may be brow ache, coryza, or a more or less general disagreeable stuffiness of the respiratory tract, generally known under the term of "hay asthma." Occasionally, as in several cases I have myself seen, there is some slight febrile reaction, the temperature mounting

as high as 101° , and this is apt to give rise to some anxiety, though it quickly subsides as a rule. Paroxysmal sneezing is another way in which the respiratory tract explodes. This sneezing either accompanies or replaces the asthmatic paroxysms, and is very well marked in a girl of fourteen now attending my out-patient department, who when seized with an attack of dyspnoea has a violent sneezing bout. Another peculiarity, which bears on a point to which I shall refer presently, is the fact that many of the patients attending my out-patient department for asthma come from the country.

In reference to these sneezing paroxysms it is interesting to note some observations of Dr. Lazarus. He has demonstrated a certain relationship between the nasal mucous membrane and the bronchial muscles, so that by the application of weak electrical currents to the nasal mucous membrane he was able to register a distinct increase in the intra-bronchial pressure. This may partly explain the common relation which exists between attacks of asthma and the occurrence of post-nasal growths, more particularly in young subjects. The removal of these adenoids or polypi very frequently entirely cures the disease.

As regards the ætiology of the disease, I have only a few words to say. It is usually stated that asthma occurs twice as much in men as in women. As regards age I may make a few remarks, because it is a commonly received opinion that asthma is a disease of advanced life. As a matter of fact, however, in a large number of the cases the disease takes origin in the first ten years of life, more in fact at that age than at any other decade. According to Salter 31 per cent. of the cases originate during the first ten years of life, and in this connection paroxysmal bronchitis of infants and young children should be mentioned. Salter relates two cases of asthma occurring in infants of fourteen and twenty-eight days, but such instances must be extremely rare. I have myself seen a very well-marked case of spasmodic asthma in a child eight months old, and children of eight to fourteen years of age often attend this hospital for the same trouble. Dr. Goodhart believes that in infancy asthma shows itself more as a bronchitis, apart from the physical signs. It comes on with suddenness, and is most usually associated with fever, generally attributed by relatives to chill. But

there are reasons for thinking that it owns a much greater variety of causes, such as over-excitement, errors of diet, dentition, and so on. Not only does it come on with great suddenness, but it clears up with remarkable celerity and certainty, and often leaves the child no worse than it was before the attack. The danger of these attacks in young children lies in the likelihood of the child's life being spoilt by the attentions of too anxious parents. In many of these cases the child is muffled up, and never allowed to go out when the air is cold, or when it is damp, but is kept in one room, generally with a bronchitis kettle on the fire. Perhaps the attack is warded off for some months, and then the case is triumphantly produced as an instance of cure by watchful care. But directly such a child gets into the air it is sure to have another attack. I believe that for a subject of bronchitis like that coddling treatment is the worst which can be adopted. I do not look upon the condition as one of simple bronchitis, but as a neurotic affection. These attacks occur in a particular class of children, namely, those who give conspicuous evidence, either of coming from a nervous stock, or who have a peculiar nervousness and excitability in themselves. In Dr. Goodhart's article in Professor Clifford Allbutt's 'System of Medicine' the remarks on this head are most instructive and suggestive, and well worthy of perusal.

As regards the influence of heredity, authorities differ with regard to this point very considerably. I must say that the experience I have had in the out-patient room of this hospital leads me to lay no stress on heredity. I cannot say that in the majority of my cases there is any history of asthma at all. More frequently, perhaps, a brother and a sister may be affected, but there is generally a history in some other members of the family of nervous disorder. Some of these patients may have a mental defect, others may suffer from neurasthenia, and so on; but I have not found it at all the rule that the parents have suffered from asthma.

As regards the cause of the paroxysm, that may be a central or a peripheral cause. It is essential to bear this in mind, because it has a very important bearing on the treatment. One must find out the actual cause of the paroxysm. Given a certain morbid sensitiveness of the nervous centres, anything may seem capable of producing an attack.

As regards the central causes, an emotional state or some unusual excitement or shock or over-fatigue of mind or body may determine an attack. The possible peripheral causes are very numerous. Dr. Kingston Fowler's classification is a very good one. They may be nasal, dental, bronchial, gastro-intestinal, uterine, cutaneous, or what he calls pneumogastric. As regards the nasal, I have already referred to the existence of adenoids, and I have seen several cases in which a youth or a girl suffering from asthma has been cured of it by the removal of adenoids or nasal polypi. It is also well known that certain odours and scents will produce an attack in some individuals, and some of these are very extraordinary indeed. Some people will have an attack of asthma precipitated by the presence of a cat in the room.

As regards dental causes, I think these are operative chiefly in infants. In some children of neurotic tendencies teething is accompanied by attacks of spasmodic dyspnoea.

With regard to bronchial cases, these are the majority, and in them the stimulus starts from the bronchial mucous membrane. And here the influence of climate and locality plays a most extraordinary part. I have to speak later of climate and health resorts; but probably instances of peculiarity of climate are familiar to you all. I know of two or three instances of people who are never well unless they are living in London, and who, as soon as they get to the country, are sure to have an attack of asthma. There is no doubt that during this month in London asthmatics do uncommonly well; in fact, of all the health resorts for asthmatic patients, I look upon London as one of the most favourable.

Gastro-intestinal affections are extremely apt to produce attacks of asthma. These affections may be flatulence, constipation, and more especially scybalous distension of the rectum, and they are common exciting causes of attacks of asthma. Under these circumstances the asthma is sometimes called peptic.

Uterine disorders may also be the starting cause of asthma, by acting as a reflex stimulus. Then, though I have not seen such cases, it is recorded that cold applied to the surface of the body will occasionally precipitate an attack, and the disappearance of certain cutaneous eruptions will also bring about an attack, more especially urticaria.

Sir Andrew Clark said that a common cause of asthma was an urticarial condition of the respiratory tract.

Pressure on the pneumogastric will produce these attacks, and this is said to be particularly the case in children, on account of enlarged glands or a neuroma pressing on the vagus, and so causing the asthma.

I have no time to enter into a full consideration of the pathology of the disease. In reviewing the various arguments which have been brought forward in favour of the different views that have been advanced, one is forced to the conclusion that there are probably several factors at work, the most likely one of which is a muscular spasm of the small bronchi and rapid tumefaction of the mucous membrane of the bronchi. It is a reasonable and satisfactory explanation of the facts, and so is the hypothesis of hyperæmia of the mucous membrane. The rapidity with which the blood-vessels all over the surface of the body may dilate under vaso-motor influence is illustrated by the act of blushing, and if the possibility of a local vaso-motor paresis be admitted it is conceivable that such a condition may occur in the mucous membrane of the respiratory tract. Salter's description of it is very exact and very eloquent, and those of you who wish to read it I refer to his works.

A few words may be necessary as regards the state of the lungs in asthma. I have already said that they are in a state of constant distension. This condition is due to several causes. First, the bronchial obstruction by spasm induces inspiratory effort. Secondly, the entering air passes the obstruction with difficulty, but the gradually increasing prolongation and force of the expiratory act shows that the air meets with still greater difficulty in escaping from the lungs. Thirdly, expansion, although prolonged, is not sufficient to equalise the quantity of air which enters and leaves the chest. A fractional addition is therefore made to the residual air by each completed act of respiration, and in time the lungs become over-distended. This condition gives rise to the well-known configuration of the chronic asthmatic.

As to the diagnosis, but little need be said. Of course it must not be mistaken for cardiac dyspnoea. A hasty conclusion may lead it to be mistaken for aneurysm, tumours, and obstructive diseases of the air-passages.

I will now pass on to treatment. The treatment of asthma is divided, roughly, into two kinds—the treatment between the paroxysms, and the treatment during the paroxysms.

First of all as regards the treatment between the paroxysms, or what may also be called preventive treatment. This is a very difficult matter, and often an unsatisfactory one, because it not only depends on the doctor's orders, but it depends more on what the patient can do and what he cannot do. It is not a matter of taking so many bottles of medicine; it is a matter of the patient's whole life, and what he eats and drinks. I said just now that the treatment is mainly climatic. Patients suffering from asthma usually come to us and say, where may I live? It is most difficult to answer that question. The only way to do it is to tell the patient he must go and find out where he can live. Every single case of asthma is a law unto itself. We can only say that the greater number of asthmatics do better in a large town than they do in the country. Some people are never well unless they are by the seaside; others are never well unless in a hilly country. But the great majority of asthmatics cannot live either by the sea or in the country; they are only free from attacks of the complaint in large towns. Not only that, but they present great peculiarities as regards the towns which they inhabit. I have found from experience that most asthmatics do well in London, but it very much depends in what part of London they live. There are several cases where even the side of the street chosen makes a difference. One patient of mine, whose home is in Yorkshire, is never well there, but directly he comes to London and lives in a certain hotel in Oxford Street the asthmatic paroxysms pass off, and as long as he resides there he is free from asthma. On one occasion he tried a nursing home in the Cavendish Square district, but the second day he was there the asthmatic paroxysms commenced, and did not relax until he went back to the hotel. So when we are asked by an asthmatic where he shall live, we have to tell him he must live two or three months in a place before finally settling down; because it is a great responsibility to tell a man he may live in London, or in the country, as the case may be, unless he has tried that particular locality for himself. In England, more particularly in the winter months, I am accus-

tomed to tell patients to try Bournemouth, Ascot, Aldershot, or Weybridge. Those places are more particularly for people with a neurotic type of asthma, the attack being of the purely "spasmodic type" without any bronchial complications. For those who suffer from asthma with bronchitis I have found Torquay very satisfactory. The south of Europe, Pau, Cimiez, Algiers, and so on for those able to travel so far. Some adults do extremely well with a sea voyage. On the other hand, I have seen seafaring men who have had to give up their profession altogether and take up their abode on land, because as long as they were at sea they were never free from asthma. Directly they came to town their attacks of asthma would entirely cease. Our usual plan in England is to send patients for treatment to the Spas in various parts of the Continent, but now we find that German physicians are treating their asthma patients during October, November, and December, by sending them to London, and it is found that during these months asthmatics in London are often free from attacks.

We have other resorts where remedial treatment is adopted. One of the most celebrated of these is Mont Dore. I saw one boy aged fifteen, an American, who had suffered from asthma all his life. He had spent a winter at Mont Dore and had stayed there till October. He benefited considerably by his stay, and was quite relieved of his asthma. He went to Lincolnshire in England, and in two days was seized with asthma. He stayed in London, in Piccadilly, for two or three days, when the attack passed off, and as long as he remained in London he was quite well. Another favourite place on the Continent is Aix-le-Bains, where such patients also do very well indeed. I have also sent young boys and girls to St. Moritz or the Engadine.

There is, as I have already mentioned, a peculiar form of treatment which is very successful indeed; but in the British Isles, as far as I know, there are only two places where it can be had, namely at Ben Rydding and at this hospital. It is treatment by a system of compressed air baths. Turkish baths are occasionally recommended for asthma, but I have not had experience of them. But of the compressed air-bath I can speak in the highest terms; I have had several patients who have obtained the very greatest relief from a course of these baths. I will ask you presently to come down

into the basement to see the bath. The bath looks like a huge diving-bell in which five or six people can sit. They go into the bath for a couple of hours. The first half hour is occupied in increasing the pressure of the air from one to nine atmospheres, at which it remains for an hour, and then it is gradually lowered to the normal. It has been found that if the alterations of pressure are made with any suddenness the patients experience a good deal of discomfort. When the pressure is first put on the patients feel an uncomfortable sensation, chiefly in the ears and throat, but they can get rid of this by swallowing some liquid. Patients, especially those suffering from the bronchial type of asthma, have told me that the relief they experience whilst in the bath is immense, and they willingly come down day after day to have their baths. The relief to them seems permanent. The air is compressed by steam power. A whistle is provided by which, if any patient feels unwell, communication can be established with the attendant, who looks in at one of the windows provided. The attendant is always close at hand while the bath is being taken, and periodically looks through the window. If an accident occurs to one of the patients, the pressure is of course lowered comparatively suddenly.

Another most important part of the treatment of asthma is the regulation of diet. The food taken by the patient must be carefully regulated. Some of the asthmatic patients who come to us we order to take stramonium, but probably find that they have been taking it frequently. We perhaps find the tongue extremely furred. It is of no use treating asthma by drugs unless the digestive functions are kept in order. One of the chief conditions is that the patients must have their chief meal at midday. It is a great mistake for a patient the subject of asthma to dine between seven and eight in the evening. If he takes the principal meal at midday, and only a light meal in the evening, he is far less likely to be awakened at night, or between four and five in the morning, with an attack of asthma. The bowels must be regulated by suitable measures, especially in old people, and it is good to put them on mercurial pills and mild laxatives. A mild alkaline bitter before meals with gentian is a favourite at this hospital. But perhaps the most valuable drugs which we can give habitually between the paroxysms of asthma are arsenic and iodide of potassium. These may be given for long periods

of time. Some authorities prefer arsenic, others iodide of potassium. If arsenic does not succeed in one case, I think I should try iodide of potassium with some sal volatile. Stramonium is very largely used, but I think it is a dangerous drug to give for a long time; it is a cardiac depressant, and sometimes acts injuriously for that reason. Quinine may also be given with advantage, and in rather big doses, for spasmodic asthma. Three-grain doses three times a day are useful.

For bronchial asthma a little different treatment has to be adopted. There is one favourite prescription which I have found very useful, namely, carbonate of soda, iodide of potassium, a little ipecacuanha wine, to which are added about fifteen minims of ethereated tincture of lobelia and ten of tincture of stramonium. I have found that as useful as anything else in the bronchial type. Stramonium, as I have said, has to be given with care, on account of its depressant action.

The usual plan in the case of a patient suffering from asthmatic paroxysms is to give some form of inhalation. I think the best consists of—

Stramonium leaves	4 drachms.
Aniseed fruit	2 "
Nitrate of potassium	2 "
Tobacco leaves	5 grains.

We place as much of that as will go on a shilling on a plate and ignite it, and tell the patient to inhale the fumes through an inverted cone. This very often gives a remarkable amount of relief; but its use should be avoided as much as possible. The worst point is that an asthmatic patient, as soon as he feels that he is going to have an attack, will at once fly to the remedy, and his state finally may be worse than it was at first, because of the irritation caused to the bronchial tract. The other drugs, such as arsenic, and iodide of potassium, and lobelia, should be tried first.

Another important drug which has been brought prominently into use during the last few years is morphia, and it is very useful. But I need not tell you that there are dangers in giving morphia to asthmatic patients, namely, dangers of inducing the morphia habit. The safest plan is never to write a prescription for morphia for the patient for the relief of paroxysms of asthma: if we do not prescribe it he cannot get it, but it should always be given by the doctor or by the nurse. For those

patients who have not had morphia before one sixth of a grain is quite sufficient, and it may be accompanied by $\frac{1}{100}$ grain of atropine. Unfortunately we generally find that we have to successively increase the dose of morphia until we have to give upwards of three quarters of a grain before any relief is experienced. Still if the patient is very bad indeed we are obliged to give something of that sort.

Another plan of relieving asthma, which is as dangerous as morphia, is the inhalation of chloroform, which also gives the greatest relief. A few drops of chloroform sprinkled on a handkerchief and held before the nose give relief in two or three minutes.

Another method of treatment, more particularly when the asthma is accompanied by bronchitis, is the inhalation of oxygen, to which there can be no objection at all. A cylinder of oxygen is placed in the room, and when a paroxysm comes on inhaling the gas for two or three minutes gives relief. I do not like giving the oxygen with an inhaler over the mouth. I prefer a straight tube from the cylinder, and arrange it so that it plays over and round the mouth and nose.

Another useful remedy is caffeine. Sometimes a very strong cup of coffee given during an attack is very useful indeed. I ought to say, however, that it is a very bad plan for an asthmatic patient to take coffee late in the evening.

Rapid Cure of Vascular Nævi in Infants.—

It is astonishing, Unna states, with what ease these arterial angiomas or venous vascular nævi can be cured with prolonged gradual compression if applied in early infancy; later it has no effect. He accomplishes this compression by painting the surface with a mixture of one part of ichthyol to nine parts collodion two or three times a day. The brown pellicle that forms compresses the nævus beneath until the rapidly growing surrounding tissues have caught up with the excessive growth of the angioma or nævus. The ichthyol collodion compression is also a rapid cure for insect bites.—

The Journal of the American Medical Assoc., April 28th.

WITH DR. TATE IN THE OUT-PATIENT DEPARTMENT, ST. THOMAS'S HOSPITAL,

THIS patient is a married woman who has had two children. Menstruation has recurred every five weeks, lasting five days. Her last confinement was ten months ago. She has pain of an intermittent and aching character. During the last three months she has experienced pain when passing her water, and she does not pass as much as she did. The bowels are constipated; there is no vaginal discharge. She complains of headache, which is more or less continuous, but relieved on lying down. The only symptoms which are referred to the pelvic organs are pain in the hypogastrium and pain on passing water. Pain in the hypogastric region may be associated with a number of conditions. It may be associated with inflammatory or diseased conditions of the uterus, or with prolapse of the organ. Then, again, we find it in some cases of chronic pelvic peritonitis or malignant disease starting in the pelvis. In other cases the pain may be due to some unhealthy condition of the bladder. Here the patient complains of pain in passing water, which may be due to some abnormal growth inside the bladder or to cystitis, or it may be caused by some new growths about the urethra. Until an examination has been made it is only possible to suggest what diseases are indicated by the symptoms complained of.

On examination the abdomen is found to be perfectly lax; there is no evidence of swelling to be felt, nor is there any undue tenderness. The flanks and lower part of the abdomen and iliac regions show no abnormal swelling. She says the pain is always relieved at the monthly periods, and this fact suggests that the trouble is connected with the uterus. The urethra is quite healthy; there is no redness nor any vascular condition about the urethra. The hymen is deficient in parts, and you can see the scars which are the result of parturition. The cervix is directed downwards, and there is a laceration right and left. The uterus can be felt through the posterior fornix. The uterus seems to be mobile, but it is retroverted, and you can feel the right ovary distinctly through the posterior

fornix. One can also just feel the lower border of the left ovary. There is a little prolapse of the vaginal wall on coughing, but nothing more. As far as the finger can tell, the uterus seems mobile; probably with the sound it will be possible to bring it forwards. All that is abnormal is retroverted uterus, with slight prolapse of the ovaries, that on the right side being more marked. It is possible that this patient will be considerably relieved by the insertion of a Hodge pessary. In some cases the presence of a retroverted uterus is associated with pain and discomfort in the pelvis, even though it is not bound down by adhesions or otherwise fixed. A patient may have a simple retroverted uterus without any symptoms whatever. The extent and severity of the symptoms produced by a retroverted uterus are rather a special individual feature. One patient will not suffer at all, and another will suffer considerably. This patient complains of pain more on the left side than on the right. On the left side there is a little resistance between the uterus and pelvic walls, and there may be a few adhesions around the uterine appendages on that side. Should this be the case, the pain this patient complains of would be relieved by rest in bed for a week or ten days, combined with hot douches, attention to the bowels, etc. With the patient lying on her back, you see that the sound passes inwards with the handle a little upwards. When the uterus is normally situated it is not possible to pass the sound up to the fundus with the patient on her back unless the buttocks are brought right to the end of the couch, so as to allow the handle of the sound to be sufficiently depressed. The mere fact that you can pass the sound in this case shows that the uterus is not normally situated. In private practice it is often more convenient to pass the sound with the patient on her side. If there are no adhesions and no inflammation of uterus or cervix, passing the sound does not usually cause pain. As the attempt to bring the uterus forwards with the sound causes this patient pain, it may be that there are some adhesions which are stretched in this act. We will introduce a Hodge pessary. If the uterus is retroverted and bound down by adhesions, you may succeed in drawing it forwards with the sound, but directly you remove it the uterus will fly back again. In the present case, after removal of the sound the body of the uterus can be felt through

the anterior fornix lying well forward, so we may feel sure that no bands of adhesion are present. This support will have to be worn for three months, at the end of which time the patient will probably be able to do without it.

The next patient is a woman aged 75. She has had eight children and three miscarriages. She complains of her womb having come down for a week, and having had a blood-stained discharge. She has a pain in her back of a bearing-down character. The menopause occurred at forty-five. There is some pain on micturition, which act the patient has some difficulty in commencing. Twelve years ago the patient had a pessary put in, which she wore for five years. It is an unusual thing for symptoms of prolapse of the uterus to start at seventy-five years of age if she has never had anything like it before. The history of having had a pessary put in twelve years ago shows that there was probably some prolapse at that time. An important symptom in this case is that she has had a blood-stained discharge for a week. In a patient of this age a blood-stained discharge is always a suspicious symptom, and calls for careful examination. If she has procidentia, with the cervix outside the vulva, possibly the cervix may be exposed, and there may be a certain amount of ulceration as the result of friction against the external parts, and as a result of that she may have a certain amount of hæmorrhage from the vaginal cervix or vaginal walls. On the other hand, there is a possibility that her symptoms of bearing down, and difficulty in passing water, with blood-stained discharge, may be due to some new growth about the cervix. Examination must be made to find out what is the condition of affairs. She has never had her uterus down from seven years ago until the present time. She has had to strain down to pass her water. We find there is complete procidentia of the uterus, with complete eversion of the anterior vaginal wall from the urethra to the cervix. With prolapse of the vaginal wall of this extent one always gets prolapse of the bladder as well, forming a cystocele. You cannot have a considerable prolapse of the vaginal wall without the bladder coming down with it. When the upper part of the prolapsed portion containing the bladder is constricted by the vulva, it is easy to see how difficulty in emptying this portion of bladder arises, and so we have an explanation of

the difficulty in micturition which she complains of. You see how there is a very unhealthy-looking posterior lip of the cervix. You have to remember that in a patient seventy-five years of age you expect to find the cervix and uterus considerably atrophied. This is seen here in the case of the anterior lip, but the posterior lip is considerably enlarged, quite four times the size of the anterior lip. In any case like that you have to say whether it is inflammatory hyperplasia of the cervix due to the portion of the cervix outside the vulva becoming inflamed as the result of chronic congestion, or whether it is a new growth. You might think on looking at it from some distance that the posterior lip of the cervix was the seat of a malignant growth, but on examining more closely you will feel that it is quite smooth, and neither hard nor friable. There has been a bilateral laceration of the cervix, and the cervical canal is widely patulous. This enlargement of the cervix is simply a result of chronic inflammatory change, probably the result, in the first place, of laceration, and secondly, to congestion in that part of the cervix outside the vulva which has been exposed. The swelling is not hard at all, as you would expect a malignant cervix to be, and it does not feel granular. If it were a malignant cervix, passing the finger over it would almost certainly cause it to bleed. It is quite certain that this cervix is not malignant. On grasping the uterus above the cervix it is found to be very small, thin, and atrophied, and it is not hard. This is one of the cases in which the feel of the cervix is absolutely diagnostic. Friction from her clothes has probably been the cause of the hæmorrhage during the past week. The easiest way of showing to what level the bladder reaches is to pass a bladder sound. That proves the anterior swelling to be a cystocele. The bladder here is rather far forwards. The sound goes in within half an inch of the vaginal aspect of the cervix. Although the anterior vaginal wall may be entirely everted in a case of cystocele—causing complete obliteration of the vaginal canal and fornix—in the case of the posterior vaginal wall you rarely get complete eversion. The present case is, however, an exception, the whole posterior vaginal wall being everted, and there is this solid brawny œdema of the vaginal wall owing to interference with the return of blood from the parts. When patients have complete

procidentia like this the ordinary pessaries keep in with difficulty. We will insert a vulcanite ring pessary, but you will see that even when this is in position the anterior vaginal wall and bladder comes down to some extent. This, however, is the best we can do for her short of operation. In a patient of this age it is not advisable to recommend operation.

The next patient is thirty-two years of age. She has had four children, the last seven months ago. The patient was in bed ten days after the birth of the last child. Six weeks after the child was born she had to go to bed on account of flooding. She had a ring put in ten days ago, and taken out last Thursday, as the vulva was sore. The patient has a slight discharge; menstruation has been regular within a day or two. She has had pain on passing water. She used to have difficulty in holding her urine. It was not till six weeks after confinement that she had her first period. Her baby died on July 2nd, and she had then excessive loss. The first period after the birth of a child or after a miscarriage may be excessive in amount. After the first her periods have returned naturally, so we do not expect there is any unhealthy condition of the endometrium which accounted for that excessive loss. The symptoms related are not very distinctive. The upper border of the right kidney can be felt, and the pain is said to be right across the loin. The pain, therefore, may be associated with movable kidney. With regard to abdominal pain, a good practical rule is that pain referred to the pelvic organs does not extend above the level of the umbilicus. Pain in the back due to pelvic mischief is usually referred to the upper part of the sacrum or the sacro-iliac synchondrosis. If there is pain above the umbilicus it is probably not due to pelvic disease. Pain across the loin is unlikely to be due to disease of the pelvic organs. The pain in the back in this patient is above the usual level of pelvic disease, and I think it probable that the movable kidney is the cause of the discomfort in the lumbar region. But she also complains of a feeling of bearing down and straining in the vulva, and it is probable we shall find something to account for that. In prolapse of the vaginal wall, in which the uterus is also prolapsed, you find the uterus retroverted. Here the uterus is anteverted. There is a little laceration of the cervix on either side, with eversion of the mucous membrane of the

cervical canal. In cases of eversion and erosion of the cervix, when there is much discharge, glycerine tampons are very useful, but they need to be applied frequently—every other day for a fortnight or three weeks—to do much good. Another useful application is glycerine and tannic acid; another is glycerine and ichthyol. Ichthyol and glycerine is very much used abroad for erosion and for cases of eversion where the laceration is not sufficiently bad to require Emmet's operation. There is little the matter with the cervix. The erosion is on the vaginal aspect. Erosion is due to a replacement of the squamous epithelium of the vaginal aspect of the cervix by columnar epithelium derived from inflamed cervical glands. So far as the pelvic organs are concerned this patient does not seem to require any special treatment.

Operations for Typhoid Perforation.

Taylor ('Annals of Gynæcology and Pediatrics,' January, 1900) reports five cases of operation for typhoid perforation. The first was operated upon after sharp abdominal pains, rigors, and vomiting, suggesting perforation. Death occurred in six or eight hours. The second case was operated on the fourth day after the accident; death ensued in eight or ten hours. The third patient was operated on twelve hours from the time symptoms suggested perforation, and recovered. The fourth patient was operated on four hours from the onset of symptoms, and died in about nine hours. The fifth patient was operated on twenty-four hours from the onset of symptoms, and practically died on the table. In at least one of these cases the early symptoms of perforation were extremely obscure. The author states that less than two hundred in all have been operated on, with still a very heavy mortality, but that nothing short of a moribund condition should warrant us in abandoning the case as hopeless. The key to success is an early operation.—*Therapeutic Gazette*, April.

MESSRS. BURROUGHS WELLCOME AND CO. are now issuing Chinosol in tabloids of five grains for internal administration. It has been given in quantities of from fifteen up to thirty grains daily in divided doses in the treatment of tuberculosis, especially in tuberculous disease of the glands and bones. It is also stated to be of service in small repeated doses in dysentery.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXIV.

GENTLEMEN,—I begin to-day the medical jurisprudence of wounds. The medical jurisprudence of wounds is indissolubly mixed up with the surgery of wounds, and the man who has the most knowledge of wounds would be able to guide a court with the greatest certainty with regard to the legal bearings of them. We need not, I think, trouble about the definitions of wounds.

Now when you are confronted with a wound on the living body, the first thing to decide is whether that wound is dangerous to life. All I would say on that head is, you must take no pedantic or ridiculous view of what a dangerous wound is. Every wound is dangerous to life, no matter how small. Cases have occurred in which septic poisoning has resulted from the prick of a pin, but you must take no such view in legal matters. You must see whether the wound is near organs or vessels which are very necessary for life, and injury to which would have been likely to cause death quickly. Then, wounds have been spoken of as "incised," "lacerated," "punctured," "contused," and so on, and in describing wounds these qualifying adjectives are sometimes necessary. Now when a wounded man is brought to you, and he has got a stab, or bullet wound, or anything else, you have to examine its exact situation—length, breadth, depth, and direction. It may be necessary for you to probe the wound; and when you probe a wound be careful to do so with very great gentleness, and it is perhaps advisable to call the attention of another medical man to the fact that you are using no force. I say that because trouble has occurred, in one case notably, where a man was shot in the neck during a Fenian disturbance in Ireland; suppurative of his wounds set in, and he ultimately died of them; and the defence here—and it is a very common defence—was that the man did not die of his wounds, but of malpraxis; that is to say, he died because the wounds were mismanaged by the surgeon, and it was sought to prove that he had used the probe with unnecessary force, his lack of skill having so increased the

damage that the man died. That happened to no less a man than Liston, who was called to a gentleman who had been wounded in a duel; the man was bleeding profusely from a wound in the thigh. Liston departed from the ordinary surgical practice of cutting down and tying the bleeding vessel *in situ*, and performed the more serious operation of exposing the external iliac and ligaturing it. The man died. When a man is killed in a duel, that is murder by the man who shoots him. The defence in this case was that he died of surgical malpraxis. It matters not whether the defence is successful or not, it is important for you to remember that that kind of defence is brought forward, and that when you are dealing with a wound, especially with a wound which has been inflicted with murderous intent, you have to exercise the greatest possible caution, and remember that a man who may be upon his trial for murder will use every possible means to get off. When you examine a wound you must note whether there has been much effusion of blood, whether the part is much inflamed, and so forth. One very important thing is this: a body may be found with wounds upon it, and you have to determine whether those wounds were the cause of death. You have also to determine whether the wounds were made post mortem or ante mortem. The determination of this is comparatively a simple matter. Wounds made ante mortem give evidence of vital phenomena; there is retraction of the edges, hæmorrhage, and, if the man has lived long enough, inflammatory action and attempts at repair. By examining the wounds you can tell whether one or other of these phenomena is present. There is no retraction of the edges of a wound made post mortem, and you know that is one of the great differences between operative surgery on the dead subject and on the living subject. But if you inflict a wound on a corpse very soon after death, before the muscular irritability has died out, then you may get a certain amount of retraction of the edges, and it is possible that if putrefaction sets in in a corpse you may get considerable hæmorrhage from divided veins.

Again, bruises undergo change of colour with age, and the presence of these changes of colour in a bruise may be important; but it is exceedingly difficult to tell the difference between the effects of time and the effects of putrefaction. With regard to wounds and bruises there is one point which you

must remember, and that is hæmophilia. Sometimes an enormous amount of hæmorrhage may take place as a result of very slight injury. I knew one case of hæmophilia in which the thigh was hit with an india-rubber air ball, and the patient had large hæmorrhage into his thigh which nearly cost him his life. There was a case many years ago, which occurred at Southampton, which caused a great deal of excitement at the time. There was a schoolmaster in whose house a boy died rather suddenly, and the coroner's jury returned a verdict of death from natural causes. But something happened which caused suspicion to gather round the case. The body was exhumed and further examined, and it was discovered that the boy had subcutaneous hæmorrhages practically all over his body, and it was shown that this was the result of the flogging he had had from his master. The boy had in fact been flogged to death. That case occurred in 1856, and caused great excitement. Not much was known then about hæmophilia, but it was conceivable that this was an unfortunate hæmophilic, and that the flogging he had had may not have been so terrible as was made out. However, the schoolmaster got a very long term of penal servitude.

With regard to wounds, I remind you that internal viscera may be ruptured and death may result without any external ecchymosis or bruising. For instance, in aguish countries spleens have been ruptured by a squeeze; and in the same way livers have been ruptured by accident, and hæmorrhage and death have resulted, though there has been very little external evidence of mischief. One point about incised wounds is that it is said an incised wound may sometimes be smaller than the weapon which made it, owing to the elasticity of the skin. That is to say, if a man gets a punctured wound with the point of a sword, when the sword is withdrawn the wound left looks so small that you would hardly think the sword could have made it. I mention it as a possible source of error. Then when a wound goes right through a body, as with a small sword, the edges of the aperture of entry are turned in, and the edges of the aperture of exit are turned outwards; but remember that when the sword is withdrawn this may be reversed. Then when wounds are made you have always to answer a suggestion as to whether they were made by this or by that weapon, or in this or that way. In a case

of abortion before the court, testimony was given that there was a wound near the os uteri, and the witness had a series of obstetrical instruments handed to him, and was asked whether in his opinion the wound which was found post mortem might have been made by one or more of the instruments which were handed to him. That is the kind of question which is very often put. Very often the suggestion is made by the prisoner that the wound has been made, not by any lethal weapon, but by falling accidentally against something which was very like a lethal weapon. For instance, a man might be knocked down and might hit himself against the projecting lock of a door. Again, a penetrating wound is said to be made sometimes by a man falling upon a nail sticking up in the floor, and so forth. I cannot lay down any exact rules for your guidance in answering questions of the sort I have indicated—whether a wound was likely to be made by an instrument of the kind shown or not. When I was house surgeon I had to attend a police court to give evidence in a case of assault, and I found I had to testify as to the wounds inflicted on a certain Bridget Macarthy. I went to the out-patient book and looked up Bridget Macarthy's case, and by the aid of this and my own memory I recalled a woman who had a series of slight wounds about the scalp, which she said a friend of hers had made with the pointed end of the snuffers. She alleged that her friend, in a moment of alcoholic frenzy, had rushed at her with the door-key and the snuffers and inflicted the wounds upon her head. When I went to give evidence, the woman appeared in the witness box swathed like an Egyptian mummy; she had on her head an incredible amount of bandage, and she told the magistrate in a voluble way how she "bled like a pig" from the wounds upon her. I began to think I had got hold of the wrong person in the records of the hospital, and I began to be afraid that I had made a mistake. However, I went into the witness box and took the bandage off on the spot, and there was nothing underneath except what I have told you; there were signs of some little wounds about the head which had got quite well.

You are very often asked to say whether the wounds found upon a dead man are homicidal or suicidal. You are generally able to give some idea on this head. For instance, a wound may be in such

a position that it could hardly have been inflicted by the dead person,—for instance, between the shoulders; it is hardly conceivable that a man could inflict a death wound upon himself by a blow between the shoulders. But here, again, you have to remember that the tricks of mad suicides are endless.

With regard to self-inflicted wounds, you must remember that a suicide who goes to cut his throat, if he is a right-handed man, takes a knife in the position I show you, and begins on the left side high up, wounding the hyoid, or sometimes higher up, and the wound is oblique from left to right and from above down. These wounds are generally deeper at the beginning than at the finish. When you find wounds in that position you will have the right to assume that they have the characters of suicide. But do not say too much. On the other hand, if a man cuts another's throat, he approaches him, and the wound is made on the right side of the throat as a rule. But here again, you must remember that a murderer has come behind his victim, and in such a case the wound may be very like that which is produced by a suicide. Again, I remember at the hospital a man who cut his throat; he was a right-handed man, and his wounds were entirely on the right side of the throat. He cut his throat with the stump of a blade of an old dinner knife, and held the blade in both hands, and persistently hacked at his throat, making a series of wounds. They had all the character of homicidal wounds. You must bear these points carefully in mind when trying to come to a conclusion.

Sometimes the character of the wound may lead to the detection of the murderer. As sheep is killed, and I believe a pig too, by transfixion; the neck is transfixed and the butcher turns the blade round. You will see that this method has been pursued if you notice a sheep in butchers' shops. A man being found with his throat cut in this artistic manner led to the detection of a butcher.

A curious case was that of the late Sultan of Turkey, Abdul Azziz. He was found dead in a bath with both brachial arteries divided at the bend of the elbow. There was a medical board to sit upon the deceased sultan, and they decided that the wounds were suicidal, and that they had been made with a pair of embroidery scissors belonging to one of the ladies of the harem. But Western

Europe was very incredulous, for it is hardly possible that a man who had divided one brachial artery could use that arm to divide the other brachial artery. It was subsequently found that he was first killed, and then these wounds were made after death. I think the sultan was smothered, but the wounds I have mentioned were not self-inflicted ones. Then you may sometimes have to consider, or you may be asked, whether a wound was accidental; and all I would say is, that I do not think you can possibly give an opinion as to whether a wound is accidental. Many years ago there was a case before our courts which caused considerable excitement, and that was the murder of a Mr. Moon by his mistress, Flora Davey. The history of the case was this. This lady and gentleman had been to the Derby, and when they came home from the Derby they had supped together, and while they were having supper some altercation arose, and Flora Davey seized the poultry carver and stabbed Moon just between the fifth and sixth ribs on the left side, making a wound five inches long, and just wounding the apex of the heart. He died, and the question was whether it was accidental, whether conceivably he had not got hold of the poultry carver, and that she seized his hands in the tussle. There were several medical witnesses in the box to say it was probably accidental. But the evidence was not credible. There was very little evidence to show that it was accidental or suicidal. Remember that you would want a rather exaggerated movement of the arm to get round to make a wound such as this was, to get the point of the knife in between the fifth and sixth ribs, and wound the apex of the heart.

Again, in a struggle a man may be wounded by a number of people, and if he dies you may be asked to say whether it was the wound inflicted by Smith, by Jones, or by Robinson which caused death. Of course you have nothing to do with who inflicted wounds, that is a matter of common evidence; but in examining the wounds you could dismiss this or that, and say they were not fatal. Then the question may arise, Did the man die of the wound or its treatment? That I have already alluded to. And then there is a statute of limitations with regard to wounds. Supposing a wound is feloniously inflicted on another person, and that person lives a year and a day, that wound cannot

be regarded as the cause of death ; that is the law. Short of that the acceleration of death by a wound amounts to murder or to manslaughter, no matter how ripe for death the person may have been before the wound was inflicted. To give a common instance, if you give a man a shove or a kick, and by so doing burst an aneurysm, that would be manslaughter, notwithstanding that that shove or kick may have been of a mild character. Or take such an instance as a schoolmaster boxing a child's ears and bursting an abscess internally, and so killing the child ; that would be technically manslaughter. It is no excuse to show that any one had a deadly disease upon him ; if you hasten his death it is quite enough.

We will go to another class of wound, namely, gunshot wounds. When you are confronted with gunshot wounds you must get every information you can. You must examine the weapon and the ammunition, and you must get a knowledge of the wadding (supposing there is any). Of course wads or wadding are very rare now, but I found a man the other day—quite a curiosity, a market gardener,—who was wandering over his fields with an old muzzle-loading shot gun, and he had a ramrod and powder-horn and a shot-flask, and he was ramming the charge home with bits of newspaper of that morning. It seems like a study from the antique, but you must remember that such things may happen. Supposing, for instance, that man had killed somebody, and you had recovered a bit of the wad, and that it was composed of that morning's 'Little Pedlington Mercury,' that would be a very valuable clue. And in former times the material of which these wads was extemporised often formed a clue to the murderer.

Now when a person is killed by a gunshot wound, you must remember that it is of importance to note the position of the aperture of entry and the aperture of exit, for that will give you a clue to the direction from which the shot was fired. By taking the bearings in that way some useful information has been obtained. There is a case on record in which a man at Greenwich was killed. Sir Astley Cooper was called to see the man who was killed. He examined the wound, and noticed the position in which the dead body was. He saw that the shot had passed through the body and hit a wall on the other side. There was a window in the direction from which the shot would come, and

Sir Astley Cooper came to the conclusion that if the person who fired the shot from the window did not wish to show his own body to the murdered person, he must have fired the shot with his left hand. I need not go into all the facts, but Sir Astley's reasoning led to the apprehension and ultimate conviction of a left-handed person. But you must remember that the apertures of entry and exit of a bullet may be very misleading. For instance, many years ago the Duke of Edinburgh (the Duke of Coburg as he is now) was shot, at an entertainment in Australia, by a Fenian with a saloon pistol. The bullet entered on the right side of the ninth dorsal vertebra, and it was found under the skin to the right of the umbilicus. It was naturally assumed at first that this bullet had probably gone right through the liver, and that the wound was almost necessarily fatal. But happily that was not the case. It had run round the rib between the planes of the muscle and the abdominal wall, and the Duke quickly recovered.

With ordinary powder there is very often blackening, and very often singeing, and the presence of blackening and singeing may be taken as evidence that the weapon was moderately close to the body when it was fired. I have here for your inspection the suit of clothing of a workman, and I ask you to notice that a large hole has been burnt through a stout moleskin jacket, a waistcoat and trouser band, both of thick corduroy, and a stout cotton shirt. The whole of that burning was the result of a pistol shot. The owner of these clothes was found dead on the floor in front of the fire, and when the doctor got to him his clothing was smouldering, and it was thought at first that he had probably had a fit, and that something out of the fire had set his clothing alight. The weapon was not discovered until afterwards. It is a case worth recording and remembering. Near where the man lay was a chest of drawers, and in the top drawer was found the pistol. Then the bullet was found, which fitted the pistol. The theory is that he shot himself while standing in front of the drawers, that he fell back in front of the fire, dropping the pistol into the top drawer, and then that the first person who came into the room after the event shut the drawer, and shut in the evidence of the cause of death. The case is interesting as showing the amount of burning which may take place. In this progressive age civilisation has given us a large

increase of murderous weapons ; that is a curious commentary on our civilisation. A great many of the modern murderous weapons are smokeless, and are made to explode with fulminates and things other than gunpowder, and there is consequently little or no blackening, and I take it that the risk of setting fire to clothing is thereby lessened.

Now I will read you a case bearing on this question of gunshot wounds, which I think is one of the very best comments in medical jurisprudence which I have ever read. It is a comment by Mr. Bowlby, of St. Bartholomew's Hospital, on the case which was known as the Uxbridge tragedy, and was tried at the Central Criminal Court on December 18th, 1884, before Mr. Justice Hawkins:—Elizabeth Gibbons, aged 54, and her husband James Gibbons, aged 67, lived alone together in a house at Hayes, near Uxbridge. They lived comfortably and were well-to-do. James Gibbons arrived home from a journey between 10 and 11 p.m., on November 15th, had his supper, and went to his bedroom. At 11.30 Elizabeth Gibbons ran for a neighbour, saying her husband had shot himself, and James Gibbons was found dead in his room, lying on his face. There was in the room a six-chambered revolver, about six inches long, which it was admitted had been purchased by the deceased. Five chambers were empty, but in the sixth was a cartridge which was jammed. This revolver had *not* what is called a rebounding lock, so that for each discharge the process of recocking would have to be performed. There were four wounds on the body, viz. :

(1) In left cheek, oblique, right to left and before back from canine tooth to mastoid process. Upper jaw shattered. Bullet found near mastoid process deep amongst the muscles of the neck. The skin round entry was blackened, but the hair not singed, so that in the opinion of Mr. Bowlby the pistol "might well have been held at a distance of several inches."

(2) Just below left clavicle, two inches outside the nipple line. Direction backwards and downwards, the bullet being found in the substance of the left subscapularis. Entry clean punched and not blackened. The hæmorrhage amongst tissues and around brachial plexus was very extensive, and the subclavian artery was found divided.

(3) On the left side, half an inch below nipple and two inches outside nipple line. Direction left to right, above down, behind forward. The fourth rib was perforated, the wound in the skin corresponded with the wound in the rib only when the arm was raised to the level of the shoulder. When the arm was by the side, the skin wound was just below the fifth rib. There were two torn holes in anterior surface of pericardium, the right ventricle was opened near the apex, there was blood in the pericardium, and the anterior mediastinum was full of blood. The bullet was found in the left lobe of the liver to the right of the ensiform cartilage. The skin round the wound was neither scorched nor blackened.

(4) Behind the left shoulder. Direction forward and a little upward ; bullet found beneath the skin on the front of the neck. Skin round considerably blackened. To make this wound the weapon must have been pointed directly forward from behind.

A fifth bullet was found in the room. This had probably been fired and missed its aim.

The clothing (shirt and jersey) showed perforations corresponding to wounds 2, 3, and 4. The holes corresponding with the shoulder wound were singed and burnt, but there was no sign of scattering of powder grains round the holes. The holes corresponding to the subclavian and heart wounds were clean punched and not singed, but there was evidence of powder scattering round the holes. Bowlby inferred (from experiments made with a similar pistol) that for the infliction of the shoulder wound the pistol must have been held about one inch from the shirt, and for the subclavian and heart wounds about six inches. As to the sequence of the wounds, the subclavian wound was probably anterior in point of time to the heart wound, because the blood had been effused with force, which could scarcely have happened after the opening of the right ventricle. Would the case be regarded as one of suicide? No, and for the following reasons :

(a) It is very unusual for a suicide to inflict a series of such severe wounds, and in this case it must be remembered that (whatever may have been the order of the wounds) each of the three wounds in front would have disabled him. The face wound would be likely to stun or stupefy him. The subclavian wound would exhaust him, and

cripple the left hand, and the heart would cause very rapid exhaustion.

(b) If the three front wounds were suicidal, the face wound was probably inflicted with the pistol in the right hand, and the heart wound *certainly* with the pistol in the left hand. The deceased was a right-handed man, and the change of the pistol from one hand to the other was at least improbable.

(c) The shoulder wound *could not* possibly have been inflicted while the pistol was in either hand of the deceased, and the theory that it was caused by the deceased falling on the pistol is in the highest degree improbable, especially when it is remembered that the deceased was found on his face.

(d) As to the heart wound, Mr. Bowlby found it impossible to hold the pistol in his own hand in the position and direction of the wound, and with the arm raised (as it must have been) without having the muzzle almost in contact with the side, which the absence of blackening and scorching of the clothing, and the scattering of powder-grains on the clothing seems to negative. It must be remembered that the pistol required recocking between each shot. Mr. Parrott, of Uxbridge, and Mr. Bowlby were agreed that the case was homicide. The jury returned a verdict of murder against Mrs. Gibbons. She was respited.

You get in that case almost all the points of importance in connection with pistol wounds, and the arguments put forward are exceedingly able. They are, perhaps, a little fine-spun for a common jury.

Now there is another very interesting case which brings in several points, and which I will therefore read to you. It was the case of *Regina v. Paul Mai*, and was tried before Mr. Justice Grove in 1872 at the Central Criminal Court. Prisoner, a German aged twenty-one, was charged with the murder of Hermann Nagel on August 21st of the same year. The prisoner and deceased arrived in London from Germany on August 10th. They entered upon a life of dissipation, and took up their residence in a brothel in Chelsea, and by the 21st all their money was gone and their valuables had been pawned, and they were destitute. On the 11th Mai had bought a pistol, and it was stated that the said pistol was always in the possession of the prisoner, and not of the deceased. In the evening of the 21st they wrote a joint letter, drank

a bottle of champagne, and went into a bedroom together, locking the door. The report of a pistol was heard, and then, after a minute's interval, a second report, and then a fall upon the ground. On breaking open the door the deceased was found lying on a sofa on his right side, with his right arm over the end of the sofa; and Mai was found at right angles to the sofa, on the floor, with his legs under the sofa. The pistol was close to Mai's left hand. The deceased's shirt had been pulled up; the prisoner's had not, but was scorched by the powder. The bullet wound extended from the left third rib in front to the eighth rib behind, and was oblique from before back and from without in Mai's wound was close to the heart. We may infer from these facts that the wounded man on the floor was wounded by the second shot (and not by the first, as stated in his "dying" declaration), and that the pistol was held close to the body in both instances, and that both wounds were *possibly* suicidal. In the absence of any evidence of the position of the wounded man at the time the shot was fired, the direction of the wound is not of much value. The case is unique as being the only instance of two persons conspiring to commit suicide with a pistol. The prisoner was acquitted on the ground that there was not sufficient evidence of their having had a joint purpose when they went into the bedroom together, notwithstanding the joint letter, in different handwriting, which was found directing one of the prostitutes with whom they had been living to take possession of sundry goods at the pawnbrokers. The man was probably just as well acquitted, but the reason for it was questionable.

Therapy of Herpes Zoster.—Bleuber ('Neurologisches Centralblatt,' No. 22, 1899) recommends an ointment made with 1 per cent. cocaine in lanolin. A thin layer is spread with the finger or cotton mop over the diseased parts and covered by a bandage. The author has treated a great many cases in this manner with marked success.—*Medical Age*, April.

WE have received from 'Punch' office a delightful volume entitled 'An Evening with Punch.' It is dedicated to "the unfortunate man, woman, or child in whose home there is not a set of the 'Punch' volumes."

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A CLINICAL LECTURE

ON

CARCINOMA OF THE PANCREAS.

Delivered at Guy's Hospital, May 16th, 1900,

By W. HALE WHITE, M.D., F.R.C.P.,
Physician to the Hospital.

GENTLEMEN,—The case we are going to take to-day is that of a man, whom I think several of you have seen, who lay for some time in Philip Ward, bed No. 14, deeply jaundiced. He was forty-nine years old when he came in, and I will give you the chief points in his history.

He was admitted for intense jaundice and indigestion, and he stated that he always enjoyed good health until six months ago. Then, about the end of October, 1899, he began to feel out of sorts and generally tired, and he lacked energy. He also suffered from extreme irritation of the skin, and this was sometimes so severe as to prevent his sleeping. There was slight jaundice in the eyes at this time, and gradually his body became yellow. About the beginning of November following he took to his bed and called in a doctor. He stayed in bed for about a week, and then got up a little, but he was not able to return to his work. By this time he was excessively jaundiced, and he noticed that he was getting thin. He thought that he had lost 2 st. in weight since the beginning of the attack. He had never had any pain during his illness at the time the note was made. When he came in, Mr. Coplands, who made the report, states—and quite correctly—that the patient was so deeply stained that at a distance of a few yards from the bed he wondered whether he was a member of a white race or not; and I must confess that when I stood at the end of the ward the same doubt passed through my mind, so deep was the discoloration. He reminded us of a member of one of the Oriental races. Then, on looking closer, we noticed that he was of an olive dark brownish-green tint, and the conjunctivæ were stained of a greenish-yellow tint. On

going into the case further we detected that his liver was uniformly and considerably enlarged; it reached nearly down to the umbilicus, and the lower edge could be easily felt. It was not painful. We next directed our attention to the bowels, and we found that the motions were constipated, and they were pale; but we did not notice anything else abnormal about them, although they were looked at on several occasions.

Turning to the urine, we found, as you might expect, that it contained a quantity of bile, but it contained no albumen and no sugar. Later on a little ascites developed in the abdomen, and he complained of considerable pain there. This came and went, and was distinctly at times paroxysmal; also the itching was a troublesome feature, and he scratched himself a great deal. After he had been in some time he began vomiting, and he had most of the signs of dilated stomach. He gradually got more and more wasted and weaker, and he died in a perfectly characteristic way—that is to say, he became slowly and gradually—so that the change from day to day was almost imperceptible—more and more drowsy, until he finally died on April 29th. Shortly before he died he is said to have had a convulsion.

Gentlemen, that is the case, and with that before us we had to try to make a diagnosis. The diagnosis was in part easy, for what we first had to consider was, what could be the cause of long-lasting jaundice getting to this deep dark brownish tint. Well, it is perhaps the easiest diagnosis in medicine, for, as far as I know, such jaundice as that means that the patient is suffering from malignant disease. The exceptions are so few that really they hardly trouble us at all. You may say that this is a rather surprising statement, because you can think of other causes of long-standing jaundice. At the bedside we commonly get three classes of long-standing considerable jaundice. By far the most common is malignant growth pressing upon the common bile-duct, and so leading to jaundice. Another common one, but not nearly so common, is an impacted gall-stone leading to long-standing jaundice; and the third fairly common cause is cirrhosis of the liver. The doctor of our patient wrote to say that he thought the patient had not told us his history fairly in saying that he had never taken too much alcohol, because the doctor knew as a fact that he had taken

too much alcohol, and therefore he had always regarded him as a case of cirrhosis. In spite of this very strong testimony on the part of the doctor, we kept to our diagnosis, namely, that the patient had a malignant growth pressing on his bile-duct, because alcoholic cirrhosis, as far as my experience goes, never produces this intensely deep dark jaundice which is caused by malignant growth pressing on the common bile-duct. The jaundice of cirrhosis never gets to that deep olive-green tint, but it remains of the yellow tint. That is not surprising, because you see there is really not much difficulty in cirrhosis in the bile getting into the intestine. It is quite an open question why a patient with cirrhosis should be jaundiced at all. It is an easy explanation, and therefore convenient for you to remember, that it is the pressure of the fibrous tissue on minute ducts in the liver leading to absorption of the bile. But this has never been proved. And when you consider how widespread throughout the body the evil effects of alcohol are, it would not be surprising if somebody were to prove that jaundice in cirrhosis is due to some defective pigment formation or destruction rather than to any re-absorption of the bile. But however that may be, the fact remains of immense importance clinically that the people with cirrhosis do not get the characteristic jaundice that this patient had.

Well, next is an impacted gall-stone. That is such an obvious cause of jaundice that it is one which candidates at examinations are constantly giving. But long-standing jaundice due to gall-stones is a rare event. Just think how exceptional it is for any one of you who have been in the wards to see such a case, and you will then appreciate the truth of what I have told you. People with gall-stones, as a rule, are liable to attacks of jaundice that come and go, not to permanent long-standing jaundice. Occasionally they do get permanent long-standing jaundice when the stone becomes impacted in the common bile-duct. Again, as far as my experience goes, those patients do not get a deep green olive tint such as do the patients with malignant growth pressing on the common bile-duct. I saw not long ago what is unusual—a patient who died from the effects of a stone in her common bile-duct. She was a lady over seventy, so that the temptation to diagnose malignant disease of her liver was very great, as she had been jaundiced for

a long while. But we hesitated ; in fact, we refused to diagnose it as malignant disease, because her jaundice was yellow, and not deep olive green. And so firmly did we believe that we were right, that—in spite of her age,—as she was getting more drowsy and obviously sinking, a surgeon was asked to open the abdomen and remove the gall-stone from the common duct. He did open the abdomen, and he found a gall-stone which was too tightly impacted for him to be able to remove it. The wound had to be sewn up, and the patient died. So you see that here we have a fatal case proved by operation to be correctly diagnosed as a case of impacted gall-stone producing long-standing jaundice. The tint was so different from that of malignant disease that even in a woman over seventy years of age we were quite willing to take the responsibility of recommending operation, in the belief that it was not a case of growth, but one of gall-stones, and we were right in doing so.

In the present case we decided that it was an example of malignant disease pressing upon the common duct. You may in passing say, might it not be something else pressing upon the common duct. I say that practically in everyday work you do not find other things pressing upon the common bile-duct, or obstructing it, except gall-stones. The text-books give you a most expansive list of things that may do it. Some begin by telling you a round worm creeping up the duct from the duodenum may cause jaundice, and all such fancy things as that, and you can, if you are ingenious, invent all sorts of things, such as enlarged kidneys and aneurysms, and cicatrising duodenal ulcers ; but clinically, in the great majority of cases the obstruction, if long lasting, is due either to growth pressing on the duct or to a gall-stone. Well, having decided that there was a growth, we had to try to get a little further with the diagnosis. Now the most likely thing was this : that there was somewhere in the periphery of the portal vein some malignant disease, and that secondary to that there was some malignant disease of the liver, and that secondary again to malignant disease of the liver there was a secondary enlargement of the glands in the portal fissure, which enlargements were pressing on the bile-duct. Therefore we searched the periphery of the portal vein ; we palpated the abdomen to see if we could find any growth ; we examined the rectum to see if we

could find any growth ; we went into the history to see if there was a growth of the stomach, but we could get no certain indication of growth in these places. So that although we left it quite an open question whether there was a growth of the periphery of the portal vein, we were not able to determine that there was. The next most likely situation for the growth is, perhaps, cancer of the head of the pancreas. You will remember that as the common bile-duct descends it passes behind the head of the pancreas, and therefore a growth of the pancreas frequently produces jaundice. We tried, but could not feel a growth in the head of the pancreas. We directed our attention also to the motions—I will return to that subject directly—and also to the points which should guide us in thinking there was a growth of the head of the pancreas. We could not get any evidence of it. The next thing that we thought of was that perhaps there might be a growth somewhere else, for instance in the gall-bladder, with secondary deposits in the glands of the portal fissure, and so pressure on the duct. We could not feel that the gall-bladder was the seat of a growth, and we had no evidence to direct us to it. The next thing it might have been was that there was a primary growth elsewhere in the body, say, for instance, in the breast, or anywhere in the body you like, with secondary deposits in the liver, and consequently deposits in the portal glands. Of that we could find no evidence. There might have been a primary growth in the bile-duct itself, but of that again we could get no evidence. It might have been that the liver was primarily affected with malignant disease. That we thought very unlikely, because that rare disease, which only occurs about once in twenty-five cases of hepatic malignant disease, generally kills the patient within two or three months. Lastly, it might have been a primary growth of the duodenum pressing on the common bile-duct ; but of that we could find no evidence. So, as in many of these cases, we had to content ourselves with the diagnosis that the patient had malignant disease pressing upon his common bile-duct. Before leaving this question I might finish speaking of the stomach. You will remember I said it was dilated. That was a point in favour of there being a primary growth in the pylorus, but it would equally well have fitted in with the supposition of a growth in the head of the pancreas,

because a growth in the head of the pancreas will sometimes press upon the pylorus so as to cause the stomach to be dilated. So the gastric dilatation did not help us much.

The diagnosis turned out to be correct as far as it went. Here are the parts removed from the patient at the post-mortem, and I have had them kept in formalin so that you could see them. You will see that this pancreas has been converted into a mass of malignant growth. It is an extremely interesting specimen, because there are two masses of growth in the pancreas. Here is the tail, which shows a large mass of growth, and here is the head, and that also contains a large mass of growth. The intervening portion of pancreas is free from growth. This large mass in the head clearly pressed on the bile-duct. You can see the gall-bladder enormously dilated, owing to pressure on the common bile-duct. But big as the gall-bladder is, you see it would not have been palpable beyond the edge of the liver, because the liver itself is so enlarged; and this enlargement is also due to pressure on the common duct, the whole liver being engorged with bile, and that is why the liver was felt during life to be uniformly enlarged. There were found to be a few secondary nodules in the liver post mortem, but they were too small to have been detected during life.

Therefore I propose now to tell you a few facts about carcinoma of the pancreas, of which this is an instance, to enable you to diagnose other cases. What I have to say is derived chiefly from our experience at Guy's. Our experience shows that it is a cause of death in about one in every two hundred patients who die in Guy's Hospital. Primary malignant disease of the pancreas is considerably commoner than the secondary deposits in it, or than extension into it of growth from some other organ. It is commoner in men than in women in the proportion of three males to one female. It is very rare to find it under thirty-five years of age, and it is very rare over sixty. The commonest decade is from fifty to sixty. You will notice that our man was forty-nine, so that he was in these respects a typical case of primary malignant disease of the pancreas. Patients are usually dead within six months from the time they first complain of symptoms, sometimes within three months, and they rarely survive to nine months.

That, I think, works out well regarding our patient, because he was taken ill at the end of October, and he died in April. So you learn the duration is commonly about six months. Pain and tenderness in the abdomen are very frequent. Our patient was peculiar in not complaining often of pain or tenderness, but he did towards the end complain of considerable pain, and, like many patients who have primary malignant disease in their pancreas, the pain was paroxysmal. Another thing which is very common in this disease is wasting, and it is a fact which is not remarkable, but is worthy of notice, that the patients waste much more than they would probably do if the malignant disease were in the breast, for instance. That is because the malignant disease of the pancreas preventing the pancreatic secretion from entering the intestine, perverts the digestion, and it is also because probably the internal secretion of the pancreas is deranged. So that the patient wastes for these two reasons in addition to the wasting that is common to all patients with malignant disease. Vomiting is very common in malignant disease of the pancreas. Our patient vomited considerably and frequently.

Then we come to a most important symptom—the symptoms mentioned hitherto are not very distinctive, although they are common—namely jaundice. Jaundice occurs in just about half the cases of primary malignant disease of the pancreas, and it is a most important diagnostic symptom. Patients usually die because of their jaundice in the same way that this patient died. You must remember that bile is a poison, and it is a poison of which the end is coma. I often ask candidates at examinations how a patient dies who has malignant disease of the liver. As a rule they do not know, and if they do they rattle off “coma, convulsions and death.” The weak part of that answer is that the coma is very much more frequent than the convulsions. This man gradually got more and more comatose, had just one convulsion, and then died. But even a solitary convulsion is exceptional. There is nothing more characteristic in our work than the death of a man who has been suffering from poisoning by bile. Day by day he lies in bed, becoming more and more comatose, more and more lifeless, his breathing is less, his pulse is less, and the life disappears so slowly that often no one but a trained doctor

could tell when life was extinct. It is one of the striking things in medicine to see these deeply greenish-yellow tinted patients, wasted, lying in bed in such a condition that a superficial observer would say that they were already dead. You cannot rouse them, they are so comatose.

A tumour is occasionally felt, but obviously the chance of your feeling an enlarged mass in the pancreas depends upon whether there is much ascites, whether the liver is big, whether the stomach is distended, whether the mass in the pancreas itself is large enough to be felt. Bearing in mind all these things that might prevent your feeling it, you are not surprised to hear that generally you do not feel the enlarged mass in the pancreas. Another symptom which is often present, and which this man had, is ascites; but it is not so frequently present as jaundice, and when present it is often, if not always, due to secondary deposits in the peritoneum setting up a malignant peritonitis; in fact I have seen primary malignant disease of the pancreas mistaken for malignant peritonitis.

The next point to which I want to direct your attention is one that is very valuable when it exists, namely a dilated gall-bladder. So valuable is it that those who write an account of carcinoma of the pancreas in their study always emphasize the importance of a dilated gall-bladder as leading to a correct diagnosis. But unfortunately it is not often dilated. It is only dilated in one quarter of the cases, and in several of those the dilatation cannot be detected during life, either because the patient has ascites, or because the stomach is dilated, or, still more commonly, as in this case, because the liver is so big that the dilated gall-bladder does not get below its edge. So that it works out, unfortunately for diagnosis, that this valuable sign, which is only present post mortem in one fourth of the cases, is not available at the bedside in more than one eighth of the cases in helping us to come to a diagnosis.

Another point that helps us in the diagnosis is that the liver is usually enlarged. That may be, as in this case, because the liver is distended with retained bile. Or it may be because the liver is full of secondary growths. The shape of the liver will tell you how to distinguish between these. If you feel lumps on the liver, it is affected with malignant disease; if the liver is perfectly uniformly

enlarged, as in this case, it is more likely that the enlargement is due to retention of bile.

Next we must say a word or two about the fæces, because they are very important. In 1820 Kuntzmann stated that in cases of pancreatic disease the fæces often contained a large amount of fat, and Dr. Bright in 1831 made the same observation, and if you read Dr. Bright's paper you will see that there is no doubt that he was unaware of Kuntzmann's previous observation; in fact most people believe that Bright was the first person to call attention to this symptom. At any rate the credit belongs to him, because he did not know that anyone else had noticed it. This is what he said:—"The symptom to which I refer is a peculiar condition of the alvine evacuation, a portion, more or less considerable, assuming the character of an oily substance resembling fat, which either passes separately from the bowels or soon divides itself from the general mass and lies on the surface, sometimes forming a thick crust, particularly about the edges of the vessel, if the fæces are of a semi-fluid consistence; sometimes floating like globules of tallow which have been melted and become cold; and sometimes assuming the form of a thin fatty pellicle over the whole, or over the fluid parts in which the more solid figured fæces are deposited. This oily matter has generally a slight yellow tinge, and a most disgustingly foetid odour." Bright himself, in the Medico-Chirurgical Society's 'Transactions,' from which this extract is taken, records three cases of malignant disease of the head of the pancreas in which this symptom was present, and he records other cases of malignant disease of the pancreas which, as he says, seems to show that it is necessary for the growth to have ulcerated into the duodenum for this symptom to have been produced. Whether or not that is so, we have not yet—although that was written seventy years ago—any evidence, because the symptom is often not properly looked for. I can only say this, that I have looked for the symptom, and I have not yet found it, but it so happens that in none of the cases in which I have looked for it has the growth ulcerated into the duodenum. So as far as that negative evidence goes it would appear to show that Bright's supposition was correct, namely that for this symptom, which undoubtedly exists, for many people have recorded it, it is necessary that the growth should have ulcerated

into the duodenum. Unfortunately, so many of the people who have recorded this symptom have not said in their notes whether or not the growth has ulcerated into the duodenum. After I have read that description from Bright, I hope you will all, when you suspect malignant disease of the pancreas, look for this peculiar fatty condition of fæces, and keep notes of what you find at the post-mortem.

Another point about the motions is that they are pale. It is stated that in pancreatic disease you are particularly liable to find in the fæces a large number of undigested muscles-fibres, from the flesh of the food that the patient has taken. And I ought, before leaving the fæces, to mention to you that Sir William Gull was of opinion that patients with disease of their mesenteric glands also sometimes presented fatty stools, such as those described by Bright in association with pancreatic disease.

Another symptom in connection with pancreatic carcinoma is that the patients are profoundly anæmic. Often a symptom which will help us is that the right rectus is rigid, in fact that is a most important symptom, as indicating that there is some disease under the muscle. It is a symptom often met with in pyloric carcinoma.

And lastly, some cases of pancreatic carcinoma have died from fatal hæmorrhage. This happened not long ago to a patient under my care. Considering the relation of the superior and inferior pancreatico-duodenal arteries to the head of the pancreas, this is not surprising.

So much for the symptoms. Now let us turn to one or two things in connection with the post-mortem. In 85 per cent. of the cases the growth is in the head of the pancreas, and in 15 per cent. it is either in the body or in the tail. It is altogether exceptional to get it in both the head and the tail with the intervening portion free, as in this case. Secondary deposits are most often met with in the neighbouring lymph-glands, in the liver, in the peritoneum, and in the lungs, and in that order. The pancreatic duct is dilated in about 12 per cent. of the cases. Therefore it will be interesting afterwards to look at this specimen, for you will see the duct is dilated very much. Sometimes the dilatation is cystic, sometimes it is uniform along the whole length of the duct. It is such a striking feature at a post-mortem that I thought it would

interest you to know what are the causes of dilatation of the pancreatic duct. I find, from collecting a number of cases, that malignant disease of the pancreas is responsible in four instances, that a pancreatic calculus is responsible in three, malignant disease of the duodenum is responsible in two, constriction from ulceration of the duodenal orifice in one, and in two the cause was not obvious. So you see the cause in the case we are discussing to-day is the commonest cause of dilatation of the pancreatic duct. Then, of course, the bile-ducts and the gall-bladder are often dilated; and you can see in this specimen that the common bile-duct is large enough to contain the index finger. We have discussed the importance of dilatation of the gall-bladder as a symptom of this disease.

The next point to which I want to direct your attention is this. You will remember I particularly mentioned, in reading out the clinical account of the case, that the urine contained no sugar, and whenever pancreatic disease is suspected you must remember to be careful to test the urine. You know from your physiology that diabetes can be experimentally produced by excising the pancreas, and clinical medicine fully confirms the belief that pancreatic disease will cause diabetes. For instance, Hausemann, from the Berlin Pathological Institute, taking a number of post-mortems at random, found diabetes without pancreatic disease in eight, diabetes without mention of pancreas six cases, diabetes with pancreatic disease in forty cases, and pancreatic disease without diabetes in nineteen cases. That gives you some impression of the great importance of disease of the pancreas in diabetes. And what we learn clinically is this, that our clinical experience entirely confirms experimental work, for the whole, or almost the whole, of the pancreas must be diseased in order that diabetes should be produced. By far the commonest abnormal condition to find in the pancreas in people who have died of diabetes is that the whole gland has undergone a sclerotic atrophy; and our records in Guy's Hospital show the same, for I find that in seventeen out of twenty instances of atrophic pancreas met with in the post-mortem room at Guy's glycosuria was present. But every condition of pancreatic disease may produce diabetes if the disease is extensive enough. Thus it has been known to occur in carcinoma of the gland; but then it must be carcinoma of the whole

gland. Therefore it is a very rare symptom indeed of carcinoma of the pancreas, because the patient is usually dead from the carcinoma in some way or another long before the whole of the gland is affected with the carcinoma, long before he gets diabetes. The fact that the whole gland was not diseased in our case explains why the patient had no glycosuria.

But then you will say, "You have not told us what form of malignant disease it is." The sections which have been cut show the growth to be a scirrhus carcinoma, and this is the commonest form of pancreatic growth. Clinically you can only tell that the patient has malignant disease of his pancreas, and nearly all cases of malignant disease of the pancreas are carcinomata. The proportion of sarcoma to carcinoma is probably one to thirty, and most likely—as is true, for instance, of sarcoma of the stomach—the patients with sarcoma of the pancreas are the young cases. For instance, out of a series of over thirty cases from Guy's the youngest patient was the only one with certain sarcoma.

I am sorry to say that in a condition such as this the treatment can only be to alleviate the symptoms as they arise.

Complete Anuria without Uræmia.—Rénon ('Giornale internazionale delle scienze mediche,' February 28th) reported to a recent meeting of the Société Médicale des Hôpitaux a case of complete anuria lasting seven days and terminating fatally, but without any uræmic accidents. The patient was an old man, aged 69, affected with right hemiplegia with secondary contraction and impairment of his faculties. His arteries were atheromatous, the heart enlarged, and the aortic orifice dilated. After some wandering pains in the right lumbar region, the patient presented complete anuria without uræmic symptoms, and died with signs of pseudo-bulbar paralysis on the third day of a second crisis of anuria. At the autopsy the kidneys were found enlarged, filled with lithic-acid calculi, the liver was cirrhotic, there was hypertrophy of the heart with atheroma of the mitral and aortic valves, and two foci of cerebral softening, the one in the left hemisphere, the other—smaller one—in the protuberance. In spite of the existence of all these lesions the patient resisted his anuria for seven days without displaying the least disturbance. This case lends support to Vidal's view as to the complex nature of the factors of uræmia.—*New York Med. Journ.*, May 12th.

THE SURGERY OF THE INTERNAL SEMILUNAR FIBRO-CARTILAGE.

A Clinical Lecture delivered at Guy's Hospital,
June 1st, 1900,

By W. ARBUTHNOT LANE, M.S.,

Surgeon to Guy's Hospital, and to the Hospital for Sick Children.

THE injury to the knee-joint to which our attention is most frequently called is damage to the internal semilunar fibro-cartilage. We are now very familiar with the varying conditions that result from bruising of this fibro-cartilage. It has always struck me as strange that, though surgeons are fully aware of the fact that this structure is very much exposed to injury, and that when bruised it becomes inflamed, as evidenced by its being swollen and tender on pressure, and by the production of an excessive secretion of synovial fluid in the joint, yet I do not think any one has ever recognised that this damaged and inflamed structure constantly forms the direct or indirect source of tubercular infection of the knee-joint.

Personally I am convinced that in the adult the large majority of cases of tubercular infection of this articulation originate in the local depreciation of vitality which is consequent upon damage to this fibro-cartilage. I have on very many occasions satisfied myself of the accuracy of this statement, and always take it into consideration when I am asked to treat cases of injury of the semilunar cartilage occurring in subjects with tubercular antecedents, or possessing some tubercular focus in any other part of the body.

I am also equally certain that damage to this structure is very frequently the starting-point of a large proportion of the cases of tubercle in the knee-joint in the young subject; but at this period of life there are other tissues which are quite as much exposed to injury.

It does not follow that the tubercular organism should of necessity first obtain a foothold in the inflamed fibro-cartilage or in the synovial membrane or tissues immediately in relation with it whose vitality have been lowered through their share in the inflammation; but the effusion in the knee-joint itself, and the general inflammation of

the synovial membrane of the articulation, which is secondary to the local inflammation of the fibro-cartilage, may form the nidus in which these organisms grow and thrive.

Therefore, contrary to the usual teaching, I would assert that tubercular affection of the knee-joint is in adult life almost always consequent upon damage to the internal semi-lunar fibro-cartilage, and in young life it frequently results from it either directly or indirectly.

I will not delay to discuss the symptoms of damage to this fibro-cartilage, since we are all very familiar with them.

I propose to demonstrate the manner in which

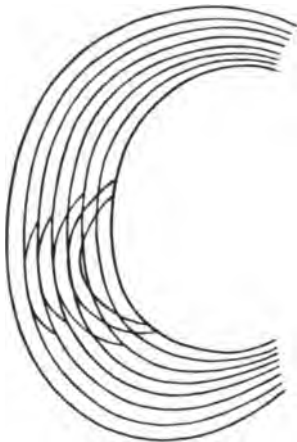


Fig. 1 represents diagrammatically the upper surface of the right internal semilunar cartilage. In it I have attempted to represent as lines running along its length the resistances offered to its elongation, and consequently to its relaxation and outward displacement, and in smaller concentric crescents the manner in which these resistances are progressively obliterated when the cartilage is nipped between the bones and severely bruised and destroyed. The force opposing the excessive suction of the fibro-cartilage between the bones is the resistance offered by the capsule and internal lateral ligament and the structures uniting the fibro-cartilage to them.

this structure is injured. I think it is clear that the cartilage is drawn in between the tibia and femur by atmospheric pressure when these bones are separated forcibly from one another, and that it is caught between them before it has time to escape on their coming suddenly into apposition. The portion of the cartilage which usually sustains injury is the inner part of the crescent just behind its centre. The destruction of this part renders the circumference of the whole cartilage greater, so that it projects outwards beyond the margins of

the bones, where it can be readily felt when swollen, and extends inwards between them to an abnormal extent when they are separated from one another. Consequently each recurring injury to the cartilage renders it more movable, and more liable to injury.

At the same time a cartilage may have been severely nipped on many occasions, and have produced on each a prolonged disability, without its showing between the accidents any very distinct evidence of damage.

After the cartilage has been completely divided the anterior portion is sucked still further in between the bones, and is cut off from its attachment to the capsule. Finally it forms a tag which has



Fig. 2 represents a right internal fibro-cartilage which has been completely divided. Its posterior third retains its normal attachment, while the remains of its anterior two thirds are shown as a tag attached by its extremity only to the tibia.

only a small attachment to the tibia in front of its spine. This plays freely about in the joint, and may even be felt external to the tendo Achillis. When this stage is reached its connection with the internal fibro-cartilage may be readily missed, since a structure resembling the original fibrous wedge is then generally pretty well developed. It occasionally happens that instead of the progressive destruction of the cartilage in the manner I have illustrated it may, in the first instance, have been sucked in so completely that pressure is brought to bear directly upon its connection with the capsule in the anterior two thirds of its extent. In such cases its continuity may be intact, while its anterior two thirds may be displaced outwards into the interval between the condyles, it only

retaining in front its connection with the spine of the tibia.

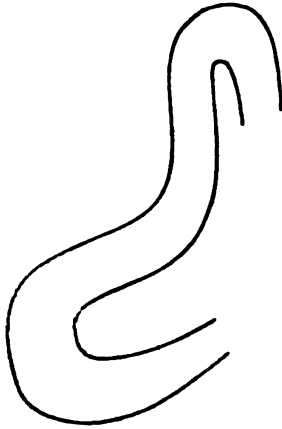


Fig. 3 represents the appearance presented by the right internal semilunar fibro-cartilage, whose anterior two thirds have been separated from the capsule of the joint, and been displaced outwards beneath the internal condyle. The anterior extremity still retains its attachment to the tibia.

Now how are the surfaces of the tibia and femur separated from one another so as to allow of the fibro-cartilage being forced in between them? This is always produced in the same manner, though not necessarily in the same position. If you stand with the knee flexed, and the tibia and fibula rotated outwards around the vertical axis, and exert pressure downwards through the head of the femur, you can readily feel that *the anterior two thirds of the inner margin of the internal condyle of the femur are separated from the corresponding margin of the tibia to an extent which varies directly with the amount of force used, and inversely with the security of the knee-joint.* This separation of surfaces takes place in all knee-joints to a greater or less extent, and results from the leverage action exerted by the long bones of the leg upon the knee-joint, force being applied vertically outside and behind the articulation of the external condyle with the tibia, which acts as a fulcrum, the internal condyle of the femur and tibia being forcibly separated. In no other position of the knee-joint can force be exerted so as to separate these surfaces, and careful inquiry into the modes in which this cartilage has been injured will show that the mechanics of the production of the injury are as I have stated.

From a recognition of the causation of this

injury our treatment must be based on certain definite principles.

First, the patient should be kept at rest after the injury with the knee-joint in a position of complete extension, in order to allow the bruised and inflamed cartilage to recover, and that its outline may be retained as intact as possible. When all tenderness on pressure on the cartilage has disappeared, and pain and effusion have quite disappeared, the muscles that control the movements of the knee-joint should be developed in order to render it secure; and its fit, as a most important bearing, accurate and reliable.

The position in which the injury to the fibro-cartilage was effected, namely, that of flexion of the knee and outward rotation of the tibia and fibula, should be avoided, and the assumption of this attitude may with advantage be controlled or opposed for a time by a suitable apparatus.

If recurrence takes place, or if in spite of prolonged rest the cartilage still remains swollen and tender on pressure, it should be removed.

In the large majority of cases I excise only the anterior two thirds or three fourths of the fibro-cartilage, since I find that the remainder causes no trouble whatever subsequently. At the same time the remainder of the cartilage can be extracted by forcibly flexing the knee, rotating the leg outwards, and separating the internal condyle from the tibia. I believe that the complete absence of any accident in the operations performed for the removal of this cartilage can be guaranteed by the surgeon never touching the wound with his hand, and not passing through his fingers the pieces of silk left in as sutures connecting the margins of the capsule. Although I have removed a great many fibro-cartilages, I do not remember any case that caused me anxiety, or in which free movement was not obtained subsequently.

We will pass on to consider some of the changes that may ensue in the knee-joint because of a direct injury.

A severe contusion of the articular cartilage covering the opposing surfaces of the femur and patella resulting from a blow on the latter bone produces just such mechanical or traumatic arthritic changes as arise in other joints from the same cause. They vary with the age of the patient, and with the force of the blow. Before middle life the tendency is towards repair of the damaged carti-

lage, a smooth surface being the result. After middle life the tendency is towards progressive destruction of the articular surfaces, the cartilage being removed in the first instance, and the bone beneath it exposed. The area from which the articular cartilage is removed is increased by frictional changes, and the exposed bone is indurated, polished, and slowly rubbed down.

Later the process extends to the margins of the articular surfaces, and lips of bone are formed upon them.

The joint becomes insecure and distended with fluid. In the vain attempt to limit its range of movement bone is deposited freely about the joint. Sometimes it is deposited in masses in the ligaments and synovial membranes. As I have described this condition, which I called *mechanical* or *traumatic arthritis*, very fully in previous papers* I will not discuss it further now, but will go on to consider the result of the direct impact of force upon the articular cartilage of the femur. In flexion of the knee beyond a right angle, there exist two considerable areas of the articular surfaces of the condyles of the femur below the patella and on either side of the ligament, which are covered merely by skin and by the capsule of the joint. In the position of kneeling with the knees separated by a moderate interval, the portion of the under surface of the internal condyle comes into immediate relationship with the ground. When kneeling on one knee the under surface of the external condyle comes into contact with the ground. In falling on one knee the same portion of the articular surface of the external condyle sustains much of the impact. If the force of the blow sustained is sufficiently great a portion of the articular cartilage is fractured and is displaced from its connection with the femur. It moves about in the joint and forms a loose body. If left for some time the sharp cut edges, both of the loose piece and the depression left by its removal, are gradually rubbed down, and I believe that after a time the loose body may be completely absorbed, and the place from which it escaped may be represented only by a slight concavity in the articular surface. It is in this manner that most loose bodies (not developed as part of a

mechanical or traumatic arthritis) are developed. I can best illustrate this by a case which I published in the 'British Medical Journal,' 2nd December, 1893. I choose this particular case for the reason that the condition existed on both sides and in different degrees of development.

E. P—, aged twenty-five, was admitted into Guy's Hospital suffering from a loose body in each knee-joint. He gave a two years' history of the trouble in the left knee and three months in the right. On opening the right knee I found a triangular bit of articular cartilage with a sharp outline, and in the under surface of the external condyle there was a gap in the articular cartilage, into which the loose body fitted so accurately that one felt much tempted to replace it. The conditions are shown very well in Fig. 4, which represents

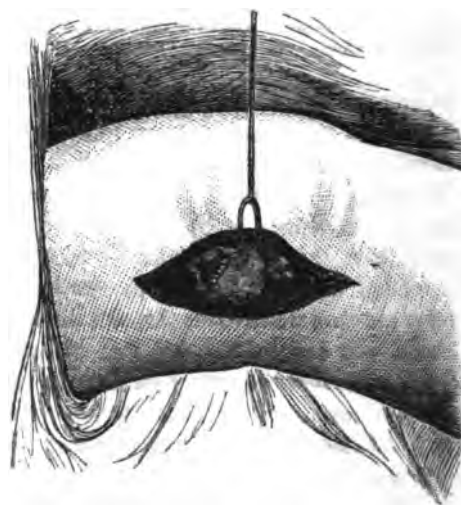


Fig. 4.

the anterior surface of the right knee flexed at an angle of 45° , and the irregular impression on the condyle into which the fragment, here separately figured in its natural size, could be fitted exactly.

A loose body of the same size and shape was found in the left knee. It differed from the other in that its margins were smooth and bevelled. On the under surface of the external condyle, in pre-

* 'Transactions' of the Pathological Society, 1886, 'Lancet,' January 30th, 1892, etc.

cisely the same position as in the other leg, there was a depression lined with articular cartilage.

As a result of force applied directly to the triangular areas of the articular surfaces of the femur below and inside or outside the ligamentum patellæ, I have seen a bruising and inflammation of one of the synovial fringes which cross obliquely the lower limits of these spaces, and associated with it considerable effusion into the joint. In one case I removed a livid mass as big as a cherry. It was gorged with blood, and was so much inflamed as almost to suggest its being gangrenous.

Effect of Slight Maternal Albuminuria upon the Child.—Vallois presents two cases bearing upon this subject. In the first a slight, transient puffiness of the eyelids was noticed at about five and a half months. The urine contained only a faint trace of albumen. An absolute milk diet caused entire disappearance of the albumen within fifteen days. On a modified milk diet the mother did well, but a slight amount of albumen returned. A strict milk diet was then adhered to, but the child died at about six and a half months, and labour took place at the seventh. The placenta shows lesions of albuminuria. Eight days after labour the urine contained no albumen. The second patient had had slight temporary œdema over the malleoli or face. At six and a half months she had general œdema, with uræmic symptoms. These disappeared upon milk diet, and a few days later a small amount of albumen appeared in the urine, and persisted until seven and a half months, when a dead and macerated fœtus was born. Albuminuric lesions were found in the placenta. A week later the albumen was no longer present. In view of the slight degree of albuminuria which may cause death of the fœtus, Vallois advises a strict milk diet during the whole of pregnancy upon the occurrence of even a small and transient amount of albumen in the urine.—*Amer. Journ. of Obstetrics*, April, 1900.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXV.

GENTLEMEN,—We pass on now to death by *drowning*. The man who is really drowned dies by inhaling water into his lungs instead of air. It does not follow that every man who is found dead in the water has been drowned, and so the definition is perhaps a little important. When a man is really drowned he dies asphyxiated; and we have to consider how a man may die who falls into the water. He may die of syncope. He may fall into the water because he faints, and if he falls into it with his circulation already stopped, when he is pulled out and his post-mortem is made you will not find the signs of asphyxia. A man who falls into the water, especially from a height, does not necessarily die from drowning; he may knock his head. People have been known to dislocate their necks by diving from a great height into shallow water. People have been poisoned, and their bodies thrown into the water afterwards in order to mislead. So that when you are confronted with a body apparently drowned, you must look it over very carefully, because you do not know whether it has been drowned or not. If the body has been drowned you will find the signs of asphyxia, and I need not say more. If the person has made, as he generally does, violent efforts to free himself of the water and to inhale air, you will find that the respiratory passages to a greater or less distance are filled with froth. That goes without saying. If a man is getting water into his air passages and then he blows air from his lungs into that water, and so backwards and forwards, he whips up the water and air into foam. Therefore the occurrence of froth, in the bigger air-passages at all events, is an important indication of drowning. If a man has been drowned, he may be full of water,—that is to say, his stomach and his lungs may be full of water. On the other hand, he may have water in his lungs, but none in his stomach. Then, if a man has been drowned there are certain external signs, *e. g.* the external signs of asphyxia—there is a blueness and ashy paleness. Then if the water has been cold you get contraction of the skin, well-marked cutis anserina, and

very commonly retraction of the penis. Then remember that "a drowning man will clutch at a straw." It is certain that a man who is drowning does clutch at anything with which his hands come into contact. It very often is found that a drowned man has in his hands something from the water in which he was drowned, such as wood, stones, or weeds; and sometimes you find mud and gravel under the nails. All these little points may be of very great importance.

Now one word about the treatment of the drowned. You are called to a drowned person, and you have to treat him. How long may a person be immersed and yet recover? Well, I do not think it is any good trying to answer that question. You hear stories of people being under for two minutes, three minutes, and five minutes, and so forth. It is an interesting speculation. But the practical point is, directly a person is hauled out of the water, do not stop to ask questions, but go to him at once and try to resuscitate him. Asking questions is merely losing time. If the case is not recoverable, that is not your fault, but if you let precious moments slip by that will be your fault.

I used to be told in my physiological lectures of somebody who allowed a bitch to drop some of her puppies directly into a bucket of water. They were resuscitated, I dare not commit myself to saying after how long a time, but I think it was hours. But then those puppies had never breathed, and probably the heart was ready to start immediately it met with some encouragement. You all know that respiration may not be going on when a baby is just born, and artificial respiration has to be performed, and is often rewarded with success. So we may take it that if a person faints when he falls into the water he will be more likely to be recovered than a person who has struggled and filled his lungs with froth and all kinds of matter which impede the entrance of air. When a person is taken from the water, remember that the first thing you have to do is to empty him. That is sometimes forgotten. It is of no use trying to pump air into a person full of water. On that account, therefore, you must be careful to get the head low. Also you must be careful to get your fingers to the back of the throat. You do not know what you are going to find there: you may find seaweed, and other substances impeding the

respiration. You must clear the respiratory passages, and place the patient in such a position that the water can run out. When you have done that, the next point is to perform artificial respiration. Now it is generally admitted that Sylvester's method is the best. You may have seen it done in the hospital over the way. In performing artificial respiration you must imitate normal respiration; you must not go at it in a hurry. A man breathes, at the most, sixteen or twenty times a minute, and you must not go quicker than that; ordinarily fourteen to sixteen times a minute is enough. Artificial respiration is purely mechanical, and if you are performing it aright you ought to be able to get evidence that air is moving through the trachea; and if it is being done aright and you apply the stethoscope to the trachea you can hear the air going in and out. This has nothing to do with recovery, it is mechanical. So that having got the patient empty and then lying on his back, you begin your artificial respiration by an expiratory movement. You stand at the head of the man, take hold of the arms at the elbows and begin by pressing the elbows against the side of the thorax, so as to give the whole thorax a squeeze, and in doing that you must squeeze the lungs and squeeze some air out. Having done that, you take the arms and bring them right back over the head, and in doing that you draw up all except the lower ribs, and you tend to produce a vacuum, and if the ways be clear, air will rush through. It is said sometimes that you ought to pull the tongue forward. Well, it is a very good thing, but I do not think it is common for the tongue to be much impediment to the entrance of air into the larynx; still it is sometimes. I remember asking a student at an examination what he would do to a drowned man, and he gave an excellent answer, stating most volubly that he would put a piece of whipcord through the tip of the tongue and tie it to his (the patient's) waistcoat button. It was not a bad notion, though the patient might resent it afterwards if he recovered. But if you have the patient's head a little on one side, there is not much tendency for the tongue to fall back. Lord Lister pointed out that in death from chloroform asphyxia, when one pulls the tongue forward it is not by removing any mechanical impediment that one does good, but by giving a sharp tug on the tongue you open the glottis reflexly.

Next, what aids to artificial respiration may we practise? Certainly, while you are performing artificial respiration, let somebody get the clothes off; then wipe the man dry, and put hot bottles to his feet, and employ friction to his limbs. You may also get some good by the injection of ether subcutaneously, and some benefit may be produced also by the injection of stimulants into the rectum. Then what good can you get from the employment of electricity in these cases? It is very doubtful if it is going to produce much good; but if you are going to use it to a drowning man you must know what you are going to do to him. If you use electricity to a drowning man it is as an aid to artificial respiration, and nothing else. That aid is brought about by stimulating the phrenic nerve and causing a violent descent of the diaphragm. The best way is to get a faradising battery and apply one theophore at the root of the neck just below the omohyoid as near as you can get, and then, when the arms are above the head during the inspiratory act, place the second theophore about the level of the sixth rib, and if you are successful you will get a vigorous descent of the diaphragm, with a full inspiratory movement. I have done it often, and have failed to do it equally often. I cannot tell you why. I ask you to remember that you may not always succeed. It is very important that if you are going to use electricity you should not leave your patient while you go and fetch a battery; but a battery may be fetched while artificial respiration is in progress, and may be very useful.

Next as to death from *hanging*. When a man is hanged, how does he die? That depends on circumstances. Hanging may be purely death from asphyxia—that is to say, almost entirely by compression of the windpipe;—but ordinarily hanging is a complicated process. In judicial hanging the executioner tries, as an act of mercy, to dislocate the vertebral column. With this object, as you know, a long drop is given. The other day a drop of ten feet was spoken of; and I need hardly say that, if you have a fairly big man and give him a drop of ten feet, your apparatus must be very strong. Care must be taken that the man's head is not pulled off, as was done once in Dublin. In that case the amount of fall was planned very carefully, and Professor Haughton was consulted, who was equally eminent as a mathematician and as

a physiologist, and he took carefully into consideration the weight of the man and the strength of the spinal column. Allowance was made for the elasticity of the rope. But the executioner was an economist, and he used a rope which had been stretched before, with the result that the criminal's head was pulled off. It was called brutality, but why it should be called so I cannot conceive. If you are going to kill a man, I do not know that it matters whether you dislocate his spinal column or pull his head off. When a man's spine is dislocated death is probably instantaneous; the respiration and circulation cease immediately. But that is not always the case. Hanging is a little different from drowning, because in ordinary hanging you are certain to get compression of the jugular veins, and therefore in hanging there is a good deal of congestion of the face. It is said that in hanging there is sometimes cerebral hæmorrhage. That I do not think could occur very often. In hanging you not only get compression of the veins, but very often there is rupture of the carotid arteries—not always of the external coat, but of the internal and middle coats. That has been found *post mortem* in these cases. It is said that there is sometimes as a result of hanging erection of the penis; but I do not find any accurate account of that phenomenon, although we know that injuries, especially to the upper part of the spinal cord, are very apt to cause priapism among other symptoms.

Now you may have to determine whether a man has been hanged before or after death—that is to say, whether death has been really due to the hanging. If death has been due to hanging you ought to find signs of asphyxia. That is a very important matter. If death has been due to hanging you ought also to find bruising and extravasation in the tissues of the neck. A tight cord round the neck leaves a mark, and that mark must differ according to the nature of the cord. If it is a stiff, hard cord, which has been pulled very tight, the pattern of the cord will probably be imprinted on the neck. On the other hand, if it were a silk handkerchief that the man hanged himself with, there would be a mark of a different kind. In old days the criminal was taken in a cart all up what was called the Oxford Road (now our Oxford Street), and hanged at Tyburn, very close to the site of the Marble Arch. The rope was adjusted around the criminal's neck, and the cart was driven on, and the criminal was

left to dangle. Under these circumstances death was very often far from instantaneous. Sometimes the sentence on the criminal was that he was to be hanged by the neck for a certain period of time. And certainly there were cases of persons escaping, no doubt with the connivance of the hangman. It is said that if a man had a good stout larynx, and perhaps ossification of the cartilages, and if the rope were artistically adjusted, he might manage to hang by the neck for a considerable time without being killed. There is a story that in the old days of the Barber-Surgeons' Company, when that corporation had the right to the bodies of executed criminals, they cut a man down at Tyburn (or perhaps it was Smithfield, for it was there people used to be hanged before they took them to Tyburn), took him into their hall, and resuscitated him. And if you go to the Barber-Surgeons' Hall you will see a very magnificent four-fold Italian screen of stamped leather, which connoisseurs say is a remarkably fine specimen. That is said to have been presented by the gentleman who, having escaped death from hanging, went to Italy, and, as a token of gratitude to the Barber-Surgeons, sent them this screen.

Now strangulation is not quite the same thing as hanging. If a man is hanged from a beam the mark of the rope must be more or less oblique, but in strangulation without hanging the course of the mark is horizontal. In Spain executions used to be effected by means of the garotte—that is to say the criminal was made to sit against a beam, and the rope was passed round the neck and tightened behind by means of a screw, and he was killed instantaneously. I do not suppose it is a bad way of executing criminals. Of course in such a case the mark of the rope would be transverse. When a man has been throttled or garroted criminally you may find the marks of finger-nails on the throat, and if you do find such marks they are important evidence.

Death may occur from suffocation. People have been smothered in bed time out of mind. Infanticide by parents who overlie their children in bed on Saturday nights is common in London. In the days before the Anatomy Act was passed, Burke and Hare were criminals who went about to get subjects for the dissecting-room; and in order to do so they employed the method which came to be known as "burking"—that is to say, they went

behind a person, closed the mouth and nose, and simply held him forcibly until he died. It is said that in some cases pitch-plasters were put over the mouth. Burking consisted in stopping the respiration forcibly, and it left little mark. However, the whole matter was ferreted out and the criminals punished.

Another form of death is by *fire*. Death from this cause is usually accidental. The interest in deaths by fire is that a body may be burnt to mislead, and the question arises as to whether you can tell whether a body has been burnt during life or after death. The answer is "Yes, in the majority of instances, but possibly not always." When a man is burnt you get redness around the burn, and a blister; that is to say, you get signs of vital reaction if the man was burnt while alive. If a man is burnt after death, on the other hand you neither get redness nor blistering. Those are two of the things to which you should look. Next, when a man is burnt how does he die? I take it that in the vast majority of cases he dies of suffocation. You cannot have a man burnt to death without his being exposed to the products of combustion; and it is probable that the carbonic oxide and the smoke render him unconscious, and he dies of asphyxia. It is supposed for instance that the physical suffering of martyrs who died at the stake was not great. Well, it is a comfortable doctrine for us to hold, but I must say it seems exceedingly likely. We know very well that if one gets into the smoke of a pile of burning wood one has got to move uncommonly quickly or suffocation will follow. It is said that the executioners could, so to say, favour the victim by making the wood a little damp, so that there was a great deal of smoke and insensibility came on very quickly. When a person dies in that way asphyxia is very quickly produced; sometimes after extensive burns you may find ulcers of the duodenum. The exact pathology of these ulcers is not very certain.

A very important form of death from the point of view of the medical jurist is death from *lightning*, and it is interesting to ask how lightning kills, and what are the signs of death from lightning? A man is killed by lightning because he becomes part of the conductor for the lightning between the cloud and the earth. Lightning may kill a man instantaneously without leaving a mark, or, on the other hand, it may cause a great deal of damage to

the body. We know that lightning obeys, broadly speaking, all the laws of electricity, one of which is that it travels by preference, if I may put it in that way, through good conductors. And when a man is struck you must remember that the lightning will take the good conductors of his body. Dry skin is a very feeble conductor indeed; if you have your hands perfectly dry, and then catch hold of the brass conductor of a battery you may feel little or nothing, but if you wet your finger it is quite another matter, because of the moisture being a good conductor. When people have been struck by lightning it has very often happened that they have been wet through. Lightning is practically always accompanied by rain, and people who have been killed by lightning have generally first been caught in a thunder shower. Moreover, people who have met their death by lightning have generally put themselves near good conductors, that is to say, they have stood under a tree which is wet through, and the trunk of the tree is a good conductor because of the moisture, and thus it comes to the wet man, who is also a good conductor. When lightning has taken a man in its course, it is very curious that if he is, as he is sure to be, of unequal conductivity in different parts of his body, it takes the good conductors by preference, such as money, or a watch chain or watch. That has been very noticeable in men who have worn a long chain round the neck, and carried the watch in a fob in the trousers; again and again it has been found that the links of that chain have been fused as well as the back of the watch, and that the watch has been stopped. Then the lightning goes down one trouser-leg, and very often makes for the metal in his boots. Bearing out that fact, which is a very important one, I will read you a most important case. It is most excellently reported, and in it you will see that all the points I have been insisting upon were borne out. It occurs in vol. xiii of the Clinical Society's 'Transactions.' Mr. Wilks, of Ashford, records that on June 8th, 1878, four labourers were overtaken in a thunderstorm. Three of them took shelter in a shed, while the fourth remained outside to pass water. Scarcely had they taken shelter when the window of the shed was blown out by a "blaze of lightning," and when the three men went to look for the fourth (named James Orman), they found the tree against which he had stood stripped of its bark, Orman's

boots at the foot of the tree, and Orman himself lying naked two yards off with nothing on him save part of the left arm of his flannel vest, albeit that two minutes before he had been completely clothed in a coarse labourer's suit and a pair of new hobnailed boots. The clothing was scattered over the ground in all directions. The man stated that while passing water he felt himself hurled in the air, but never lost consciousness. The eyebrows, whiskers and beard were scorched, the chest and belly covered by branching burns. Down each thigh and leg was a long burn. In the right heel was a wound through which could be felt the comminuted *os calcis*. There was a compound fracture of the right leg. The man made a good recovery. As regards his clothing, the jacket was split in halves, the shirt torn to rags, and burnt where it had touched the buckle of the waist-belt and the watch in the fob. The flannel vest was torn, the trousers were in two bits, all below the knees being torn to ribbons. The belt had the buckle burnt out, the knee straps were burst. The boots had completely lost their laces, the eyelets burst in places, the leather torn, the right sole rent, and the right heel tip partly torn off, with one nail missing. The watch was burnt right through "as with a soldering iron" and the chain almost destroyed. Mr. Wilks directs attention (1) to the almost complete immunity of the nervous system, due probably to the high conducting power of the wet clothing; (2) wherever there was a piece of metal there was development of heat; (3) the man was aware that the heel of his right foot was habitually raised when passing water.

That is a very interesting case, and the damage in the heel was probably due to the break in the continuity caused by his raising the right heel in the act of passing water. I show you a photograph of the clothing, which is in the College of Surgeons' Museum. You will notice also that in the account there is mention of branching burns—dendritic burns—upon the body, and all kinds of speculations have been made about these appearances. They have been said to be photographic marks, and so forth; but I should be inclined to think that the most probable explanation is that these markings correspond with the lines of sweat. We know that if a man comes to consult us in the summer, and you ask him to take his clothes off, if you look carefully you will see sweat markings

all over his body in the grooves left by the folds of the clothing. My idea is that the electricity follows the lines of greatest moisture.

Now death from lightning is tolerably common, and supposing a man is found dead in the open, it is very desirable that you should be able to form some conclusion as to whether death took place from lightning or not. But there have been cases of death from lightning where there has been nothing to show; there have also been cases of death from lightning where the lightning has made a hole like that produced by a bullet. Now in these latter years not only have we to deal with death by lightning, but also from high tension electric currents. Electricity is now omnipresent, and the question is, can we come to any conclusion supposing we find a man dead, as to whether his death may have been caused by the electric current or not? I need hardly remind you that in America they have been trying to execute criminals by the high-tension electric current, which they call "electrocution." This subject has been worked out by Dr. Thomas Oliver, of Newcastle-on-Tyne. Alternating currents are said to be most dangerous, but the evidence on that point is a little short. The causes which predispose are damp clothes, sweating hands, and the carrying of good conductors. At St. Peter's, Newcastle, in January, 1897, a youth carried an iron rod through a factory, and accidentally brought the top of it in contact with the terminals of an arc-lamp, and he was killed instantaneously.

The symptoms are first of all a sudden tetanic rigidity of the muscles, so that a patient cannot relax his grasp. There are sudden pains, local burnings, sometimes followed by sloughing. A peculiar cry is often uttered, generally when the contact is broken. The patient is found pale, slightly cyanosed, pulseless, with mucus escaping from the mouth and nose; there is now and then gasping respiration and the pupils keep dilating. Artificial respiration must be resorted to at once. This also would be the treatment in supposed death by lightning. Rigor mortis is usually marked. That is important, because John Hunter found that cows which had been killed by lightning had not rigor mortis in a marked degree; but it is probable that it had occurred and passed off before he tested them. The abdominal viscera and large veins are deeply congested, heart flaccid.

Right heart full of blood, left empty; brain engorged. Pupils widely dilated immediately after death. Blood contains oxyhæmoglobin and reduced hæmoglobin. Oliver records a case in which the heart of a dog resumed its contractions after a quiescence of thirteen minutes, showing that artificial respiration is not to be lightly discarded. When you go in for artificial respiration in any of these conditions you must be persevering, and your perseverance will not infrequently be rewarded.

Perineal Resection of the Rectum for Carcinoma.—By Dr. Halstead. ('*Amer. Journ. Med. Science*,' cxix, No. 3). The author reports three cases operated on by this method. He states that there has not been sufficient time as yet for him to determine the final results of the operation. They do, however, illustrate the value of this method of procedure. The operation is an old one and bears the name of Lisfrance. It should be employed in all cases of cancer of the rectum situated below the peritoneal fold. The mortality from it is not as high as from other operations, and the permanent results are better than in the sacral method of rectal excision. The peritoneal method has an immediate mortality of 18 per cent., and the percentage of permanent cures is small. The author believes the perineal operation is the easier to perform and is not followed by so high a mortality, and that the functional results are better. After this operation, good control of the bowel is to be had when the levatores ani are sutured to the stump after amputation.—*Post Graduate*, April, 1900.

Formaldehyde Fumes in Whooping-Cough.—Harrington ('*Annals of Gynæcology and Pediatrics*,' No. 10, 1899) states that formaldehyde fumes will completely check the vomiting and control the paroxysms of cough. A sufficient amount of gas for an ordinary room is generated by placing two or three paraform tablets in a half drachm of alcohol and allowing them to evaporate over a gentle heat. If the fumes become irritant they will do more harm than good. In very severe cases he also recommends the administration of bromoform.—*Interstate Medical Journal*, May, 1900.

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ON NEUROMATA AND ALLIED CONDITIONS OF THE SKIN AND NERVOUS SYSTEM.

BY

J. BLAND-SUTTON.

FOR some years the attention of observers concerned with pathological histology has been closely directed to a set of peculiar conditions affecting the skin and peripheral nerves which, at first sight, seem to have nothing in common so far as their clinical aspects are concerned, namely, tumours on nerves and the curious pendulous folds of the skin known as molluscum fibrosum. In 1894 I published a book on Tumours, and during its preparation I was sorely puzzled how to classify the tumours known as neuromata and fibromata, notwithstanding the fact that Professor von Recklinghausen had shown the pathological association of discrete molluscum nodules and multiple neuromata. I was able, however, to refer to an important case, reported by Lamprey, in which discrete molluscum nodules and pendulous folds co-existed. For some years German surgeons have recorded, from time to time, a singular condition to which nerves are liable, known as "plexiform neuroma." This is a somewhat rare tumour-disease, and one in which I have long been interested. Specimens have been described in this country under various titles, because the effects upon the nerve are so striking, when well marked, as to excite the curiosity of those into whose hands they fall. Though several of these specimens have been made the subject of careful study, and in some instances preserved in museums, nevertheless the "plexiform neuroma" has not been generally recognised, nor has its nature been fully appreciated in England.

This year a valuable monograph *On Neuroma and Neuro-fibromatosis* has been published in Edinburgh, by Alexis Thomson, and it may be stated that no work equal to it in importance in

relation to neuromata has appeared since Smith's classical monograph, *On Neuroma*, was published in Dublin in 1849.

The object of my present communication is not only to draw attention to Alexis Thomson's excellent work, and to show that my observations are in harmony with his, but also to indicate that the generalisations should be extended so as to include the central nervous system and those parts derived from the prolongation known as the optic stalk—to wit, the retina and the optic nerve. I propose to deal with the subject systematically, beginning with the discrete tumours arising on nerve-trunks, to which the name neuromata has been applied by surgical writers, as well as the authors of monographs on tumours of nerves, for the last one hundred years.

NEUROMA.—This may be defined as a tumour growing from and in structure resembling the sheath of a nerve.

The term neuroma is often used, especially in clinical work, as signifying a tumour on a nerve, but as they are sometimes composed of fibrous, fatty, or even sarcomatous tissue, it would be better to speak of them as lipoma of a nerve, sarcoma of a nerve, and so on.

The tumours which most strictly correspond to my definition are those known as neuro-fibromata, and it will be convenient to include the curious nodule known as the "painful subcutaneous tubercle" described by Wood in 1812.

A neuro-fibroma is usually fusiform, and grows from the side of a nerve: when large it may spread out the fasciculi of the nerve: exceptionally the nerve-fibres will traverse the tumour. The long axis of the neuroma coincides with that of the nerve from which it grows.

In size neuro-fibromata vary greatly; some are no larger than lentils, others may be as big as a fist; larger specimens are very exceptional. They occur on the cranial as well as on the spinal nerves, and form on their roots, trunks, branches, or the terminal twigs. Neuro-fibromata form smooth swellings, which are mobile, and when situated in the subcutaneous tissue glide easily under the skin; they are encapsuled and may be easily enucleated; are extremely liable to become myxomatous, and in large specimens this change leads to the formation of cavities in the tumours. These changes account for the various names

applied to them, such as myxoma, myxo-fibroma, myxo-sarcoma, and the like.

PAINFUL SUBCUTANEOUS TUBERCLE.—This term was applied by Wood in 1812 to a small discrete nodule which forms in the subcutaneous tissue. It is usually of the "size and form of a flattened garden pea," but it very rarely exceeds the size of a coffee bean. When examined by the finger it feels like a small shotty body slipping about immediately beneath the skin. Structurally the "tubercle" consists of fibrous tissue very like that which constitutes the bulk of the nodules in molluscum fibrosum; it is rare that a nerve-fibril can be traced to it.

The interest of these bodies is due to the "very severe and excruciating pain" associated with them. The pain is paroxysmal, and usually increases in severity and in frequency according to the length of time the disease has existed. If the "tubercle"—for it is usually solitary—is struck or even touched acute pain is produced.

They occur much more frequently in women than in men, and are commonly met with in early adult life: and though a "tubercle" may form on any part of the body, it shows marked preference for the lower limb. Excision of the little body at once and permanently arrests the pain.

GANGLIONIC NEUROMA.—This is a tumour composed of nerve-cells, nerve-fibres, and neuroglia. They are extremely rare tumours. Klebs described a tumour of this kind which grew from the floor of the fourth ventricle near the calamus scriptorius. The tumour was nearly as large as a walnut. It has been thought that some tumours described as gliomata may have been ganglionic neuromata: on the other hand, however carefully the histologic features of these tumours have been described, there has always been a doubt lest normal brain tissue became included in the tumour. However, this cloud has been dispelled by the observation that tumours containing ganglionic tissues occur in connection with the great cords of the sympathetic system as well as in the subcutaneous tissue.

One of the most remarkable cases is recorded by Knauss, in which a girl eight years of age had sixty-three tumours in the subcutaneous tissue of the trunk and thighs: they varied in size from a pea to an orange, were firm and elastic, and not

painful. Microscopically these tumours were found to be composed of ganglionic nerve-cells, medullated and non-medullated nerve-fibres. Knauss believed that these tumours were derived from the minute ganglia on the finest terminal fibres of the sympathetic system which accompany the blood-

cells have been found in ovarian dermoids. Other examples have been observed in connection with the sympathetic cord, and plexuses in the abdomen. The chief of these have been collected by Alexis Thomson.

NEURO-FIBROMATOSIS.—Under this heading it is now necessary to describe several affections which were formerly regarded as being quite distinct. These are multiple neuromata, molluscum fibrosum, plexiform neuromata, sarcomata of nerves, and glioma. It will be useful to state a few facts concerning each of these conditions before de-

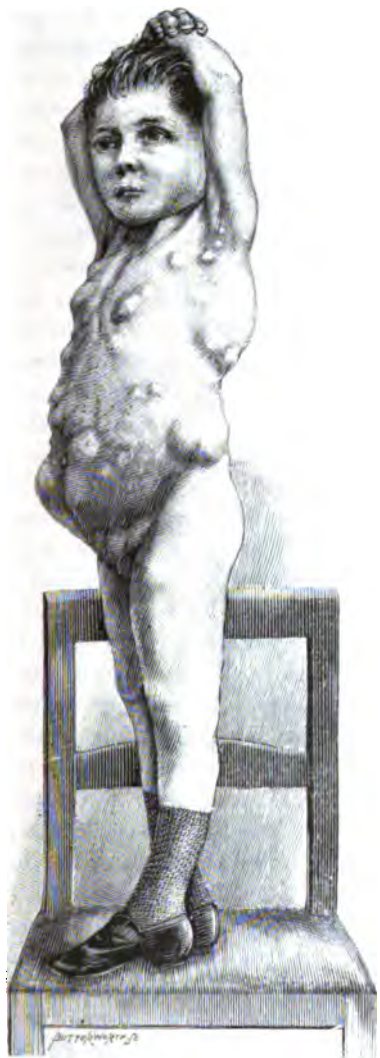


Fig. 1.—Girl eight years of age, with sixty-three ganglionic neuromata in the subcutaneous tissue of the trunk and thigh (Knauss, in 'Virchow's Archiv,' Bd. cl., S. III).

and lymph-vessels. Knauss's description of the microscopic characters of the tumours, which clinically resembled lipomata, is accompanied by careful drawings. This observation is of great interest, as it may help to explain some of those very rare instances in which collections of nerve-



Fig. 2.—Multiple molluscum fibrosum combined with tumours on the brachial plexus (after Payne).

scribing their intimate relationship. It has long been known that neuromata sometimes occur on nerves in extraordinary numbers. A remarkable case is described in Smith's classical monograph, in which a man named Michael Lawlor had at least 2000 tumours connected with his nerves. There were 450 counted on the nerves of the right lower limb, and 300 on the left. There were 200 tumours on the right, and 100 on the left upper limb. The vagi and their branches possessed 60

tumours, some of large size. The remainder were on the trunk.

Several cases of this kind have been carefully described, but probably in no individual has a greater number of nodules been detected.

In 1882 Professor von Recklinghausen published an important monograph, in which he demonstrated not only that multiple neuromata were sometimes associated with molluscum fibrosum, but the two conditions were closely related.



Fig. 3.—A native of Sierra Leone, aged fifty years, with molluscum fibrosum (Lamprey).

Dr. Payne was among the first in this country to study the disease in the new light afforded by von Recklinghausen's exhaustive work, and he published a very careful account of a case in the 'Transactions of the Pathological Society,' vol. xxxviii. The patient was thirty-seven years of age; the cutaneous nodules were very numerous, the skin itself was pigmented, and there were several

tumours on the brachial plexus. The careful description of the microscopic characters of the skin nodules is interesting, because Payne does not accept the views of von Recklinghausen entirely, and thinks that some of the molluscum bodies appeared to be formed around other structures of the skin, such as glands and hair follicles.

In typical cases of *molluscum fibrosum* the skin of the trunk and limbs presents numbers of small tumours, consisting mainly of fibrous tissue springing from the subcutaneous connective tissue. These tumours are of various sizes, some being no larger than a pin's head, whilst many are as big as a filbert, and a few even larger. The majority are about the size of a small pea. Many are sessile, and others are distinctly pedunculated, but all are covered with skin. These tumours are mobile, soft to the touch, and of the consistence of firm fat. Sometimes the disease affects a broad area of skin on the head, trunk, or limbs, causing it to hang in pendulous folds. Exceptionally the pendulous and nodular lesions occur in the same patient (Fig. 3). Lamprey, who reported the case in the 'British Medical Journal,' 1892, vol. i, p. 173, states the nodules varied in size from a peppercorn to a billiard ball; some of them had ulcerated. The man stated that he was born with lumps in his skin. The congenital existence of the lesions is a constant feature, whether the disease manifests itself in the form of nodules or in the form of localised folds (Fig. 4). The disease in this man was thought when he first came under observation to be probably elephantiasis, and careful search was made for filariæ, but without success.

Professor von Recklinghausen not only clearly detected the association of the two classes of tumours, but he urged that the molluscum bodies of the skin were formed on the cutaneous nerves, and were as truly neuromata as the fibrous tumours on the epineurium of the deeper nerves. His views have been ably criticised by Payne, but it is clear that molluscum nodules are due to fibromatosis of the terminal filament of the cutaneous nerves.

In its mildest form molluscum fibrosum appears as a single pedunculated tumour, the most frequent situation being the labium majus (Fig. 5).

The structure of these solitary tumours is the same as the nodules in the multiple forms and the pendulous skin folds. An unusual situation is the

mammary areola (Fig. 6) or the nipple. When these nodules grow from the nose they are apt to be confounded with the condition commonly, but erroneously, called "lipoma nasi." This is shown in the photograph of the man (Fig. 7) who had a number of molluscum nodules on the scalp, pinna, nose, and fingers. The disposition of the nodules on the fingers follows in a marked manner the course of the digital nerves.

sensation of women with pendulous folds and a multitude of cutaneous nodules, under the title "fibrosum molluscum multiplex." The disease appears to be equally common in men and women. It is necessary to mention that when the lower limb is the seat of molluscum fibrosum the enlargement often causes the limb to assume the peculiar characters seen in elephantiasis arabum, but of course there are no filariæ in the blood.



Fig. 4.—A woman forty-five years of age with molluscum fibrosum; it was noticed at birth. The tumour was successfully removed by Mott.

Concerning the cause of molluscum fibrosum nothing is known. The disease is not confined to any clime or race, for it has been observed in North America, the British Isles, Germany, and the West Coast of Africa. It has nothing in common with the local cutaneous enlargements produced by filaria (elephantiasis arabum).

Many cases have been reported under a variety of names, *e.g.* fibro-cellular tumour, dermatolysis, and pachydermatocele. The frontispiece to Virchow's *Die Krankhaften Geschwülste* is a repre-

Many such cases have served as clinical puzzles in consequence.

An important feature connected with the typical generalised neuro-fibromatosis is the liability of the patients to sarcoma; this may develop primarily, or arise as a malignant change in a molluscum nodule which has existed very many years. Sarcomata of this kind do not, as a rule, disseminate.

In the generalised neuro-fibromatosis death often results from gradual exhaustion, and this is sometimes hastened by ulceration, septic changes,

or sloughing of the pendulous portions of the skin.



Fig. 5.—Pedunculated myxoma from the labium majus of a woman fifty years of age; it had existed many years (from Bland-Sutton's 'Tumours').

In many cases some intercurrent malady supervenes, such as pneumonia; in the patients with

In regard to sarcoma supervening in the so-called molluscum nodules, it is necessary to remember that spindle-celled sarcomata arise primarily in nerve-trunks, especially in the great sciatic and its branches, quite apart from the existence of neuro-fibromatosis, localised or general. A sarcoma of a nerve recurs after removal or amputation of the limb, but dissemination is not frequent, and probably deferred to quite the late stages.

THE PLEXIFORM NEUROMA.—The peculiar condition to which this term has been applied is essentially a fibromatosis confined to a particular nerve or plexus of nerves. Although it is a rare condition, a sufficient number of cases have been carefully observed and recorded to enable a fairly complete account of the disease to be written. A plexiform neuroma, instead of forming a distinct tumour, as in the case of the solitary neuro-fibroma, appears as if the branches of a nerve distributed to a particular area of the skin became enlarged and elongated. The overlapping skin becomes stretched, thinned, and raised over the thickened nerves; the overlying skin is often pigmented, the usual colour being brown, like that characteristic of the hairy mole. Occasionally the skin is coarse and thick, as in the case of a molluscum nodule.



Fig. 6.—Pedunculated molluscum fibrosum from the nipple of a woman (Museum, Middlesex Hospital).

multiple nodules on the roots of the spinal nerves, one of them may so enlarge as to press on the cord and produce fatal paraplegia. This mode of death occurred in the patient whose cauda equina is represented in Fig. 11.

The tumour feels like a bag containing a number of tortuous, irregular vermiform bodies, soft to the touch and mobile. These bodies vary in thickness from a crowquill to that of the thumb; manipulation does not produce pain,

though the lumps themselves are sensitive. When the skin covering the tumour is reflected these elongated bodies will be found to lie in the direction of the nerve distributed to the part. Thus on the back they will run in a transverse direction (Fig. 8), whereas on the scalp they will trend to the vertex, and so on.

When these thickened nerves are divided the enlargement will be seen to be due to the presence of a gelatinous tissue, and the appearance of the cut surface reminds one of the umbilical cord.

represent in transverse sections portions of the great occipital nerve of a girl twenty-one years of age affected with a plexiform neuroma. The scalp overlying the affected nerve was transformed into a brown hairy mole. Widely different opinions are held by equally competent observers in regard to the effects of these changes in the sheath upon the axis-cylinders of the nerves. Some maintain that degeneration occurs, and others that they are not affected. This question requires careful investigation.



Fig. 7.—Man aged thirty with molluscum nodules on the head and hands.

Microscopic examination shows that this thickening is due to overgrowth of the connective tissue of the nerve-sheath, and especially that part of it known as the endoneurium, that is the delicate connective tissue between the individual fibres of a nerve bundle. The enlargement is by no means uniform, so that the so-called multiple neuromata are really due to local irregularities in a diffuse overgrowth of the connective tissue of the nerve-sheath. This is shown in Figs. 9 and 10, which

The diffuse character of the enlargement in plexiform neuromata is well shown in a remarkable specimen preserved in the Middlesex Hospital Museum (Fig. 11). A man forty-five years of age was admitted into the hospital with well-marked paraplegia. At the post-mortem examination a large number of small tumours were found on the roots of the nerves. Many of the roots were so beset with these tumours as to resemble strings of beads. In the cervical region there was a tumour as large

as a nut, which had compressed the cord and produced paraplegia. There was a large neuroma on the anterior crural nerve, and smaller examples on the branches of the lumbar plexus. When these nerve-roots are carefully examined they present the annulated appearance so characteristic of the root of the ipecacuanha plant, and it is clearly seen that the nerve-roots are thickened throughout, and that the nodosities are local ex-

cutaneous branches of the nerve are very thick and irregularly nodulated. The microscopic changes in the musculo-spiral nerve are identical with those found in the thickened nerves of a plexiform neuroma underlying the pigmented mother-marks. An interesting feature of this specimen is the large smooth ovoid tumour which occupies the bend of the arm, and is attached to one of the branches of the musculo-spiral nerve,

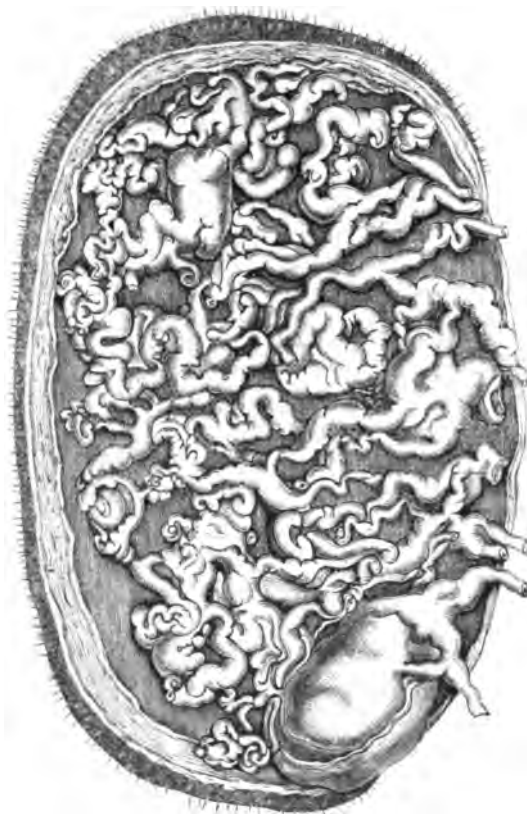


Fig. 8.—Plexiform neuroma from the back of a youth aged nineteen years. The skin was the seat of a brown hairy mole (Bruns).

aggerations. The details of this case were recorded by Sibley in 1866 (*Med.-Chir. Trans.*, vol. xlix, 39).

Campbell de Morgan published the details of a case in which the musculo-spiral nerve and its branches in the forearm were attacked. The patient, a girl of eighteen, had the forearm amputated. The dissected specimen is preserved in the museum of the Middlesex Hospital (Figs. 12 and 13). The musculo-spiral nerve is as thick as the thumb; it looked gelatinous, like an umbilical cord. The

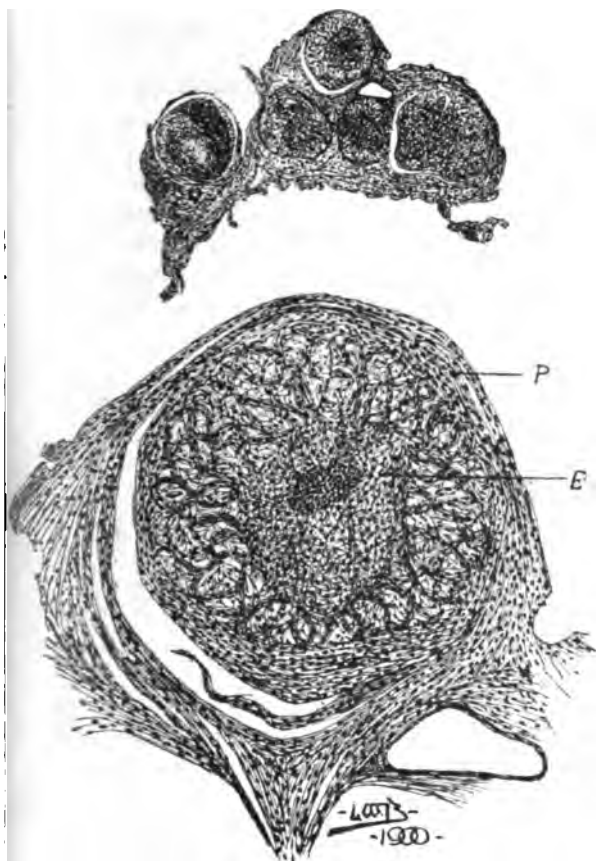
and will bear comparison with the large mass on the anterior crural nerve in Sibley's case. Any nerve is liable to this form of fibromatosis, cranial or spinal. In the cranial set the disease shows marked preference for the vagus and the trigeminal. The roots of the spinal nerve, as is demonstrated in Fig. 11, are by no means spared, and those constituting the chorda equina seem to be especially liable.

Alexis Thomson has shown that the branches of

nerves distributed to muscles also display nodosities within the muscles, and we know that the sympathetic plays an important part, for the nerves composing the great lateral cords, as well as those belonging to viscera, such as the lungs and liver, are thickened and nodose when attacked by this disease.

More than one pathologist who has carefully

this matter with me, but a critical microscopic examination of some thickened roots which he



Figs. 9 and 10.—Microscopic characters of a nerve from a case of plexiform neuroma affecting the great occipital. The upper figure represents the natural size of the affected nerve. The lower one shows a single nerve strand highly magnified. E, endoneurium; P, perineurium.

studied the annulated thickenings of nerves, particularly those on the roots of the spinal nerves, has been struck by the notion that the annulations might possibly be due to thickenings of the tiny twigs of the sympathetic distributed to their sheaths, the nervi nervorum, and that they might be enlargements of the Pacinian-like bodies occasionally found on the branches of the nervi vasorum. I remember Mr. Shattock once carefully discussed



Fig. 11.—Plexiform neuroma affecting the roots of the chorda equina and anterior crural nerve (from Bland-Sutton's article "Tumours" in Treves's 'System of Surgery').

carried out did not support the view; the same idea had also occurred to Virchow.

A careful study of the recorded cases shows that the so-called plexiform neuroma may exist in association with any of the cutaneous lesions appearing in generalised neuro-fibromatosis.

I have seen a plexiform neuroma of the great occipital underlie a brown hairy mole of the scalp, and also one affecting the branches of the anterior tibial nerve, distributed to the dorsum of the foot; the overlying skin was brown but hairless. The patient, a boy of ten years, was under the care of the late Mr. Hulke, who attempted to remove it. Battle published the details of a case in which he removed a large molluscum fibrosum from the thigh of a girl aged eighteen. The base of the tumour was occupied by thickened nerves ('Lancet,' 1896, vol. ii, p. 175).

The only example I have seen, in which discrete molluscum nodules were associated with the peculiar annulated thickening of the peripheral nerves

ing tissue: the gliomatous tissue may have the consistence of the vitreous, or be as firm as the tissue of the pons. Microscopically it has the characters of an overgrowth of neuroglia.

Virchow pointed out that when a glioma is situated near the surface of the cerebral cortex it appears like a colossal convolution. Should it grow in the tissue of an optic thalamus this structure will bulge into the third ventricle as though overgrown and a glioma of the occipital lobe will project into the descending cornu like an additional thalamus. The best illustrations of this indefiniteness, so characteristic of gliomata, come out very strikingly when the pons and the cerebral crura are occupied by this form of tumour.

Gliomata occasionally occur in the pons, and form tumours of considerable size. Sometimes they are confined to one side, and extend into the adjacent cerebellar crura. In a case described

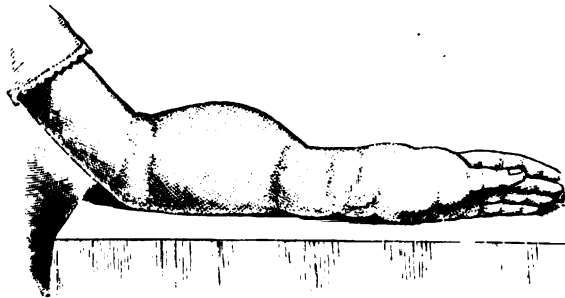


Fig. 12.—Arm in which the musculo-spiral nerve was neuromatous (from 'Trans. Path. Soc.,' vol. xxvi.)

characteristic of plexiform neuroma, was a man twenty-three years of age in the Middlesex Hospital, under the care of Dr. Coupland (see 'Trans. Clin. Soc.,' vol. xxx, p. 221). In this case the patient had a large frontal osteoma and symptoms of intra-cranial pressure; it was difficult to decide whether the head symptoms were due to the osteoma or the fibromatosis of cranial nerves in their intra-cranial course.

GLIOMA OF THE BRAIN AND SPINAL CORD.—Ever since I became practically acquainted with the changes in the nerves of the so-called plexiform neuroma, it seemed to me that the change was akin to the localised neuroglia overgrowth in the brain known as glioma, and I was sufficiently convinced of this to draw attention to the likeness in my book on Tumours, published in 1894.

A glioma of the brain occurs as a translucent swelling imperfectly demarcated from the surround-

ing tissue: the gliomatous tissue may have the consistence of the vitreous, or be as firm as the tissue of the pons. Microscopically it has the characters of an overgrowth of neuroglia. Virchow pointed out that when a glioma is situated near the surface of the cerebral cortex it appears like a colossal convolution. Should it grow in the tissue of an optic thalamus this structure will bulge into the third ventricle as though overgrown and a glioma of the occipital lobe will project into the descending cornu like an additional thalamus. The best illustrations of this indefiniteness, so characteristic of gliomata, come out very strikingly when the pons and the cerebral crura are occupied by this form of tumour.

Gliomata occasionally occur in the pons, and form tumours of considerable size. Sometimes they are confined to one side, and extend into the adjacent cerebellar crura. In a case described by Dr. Wm. Cayley, which occurred in a child two years of age, a glioma as large as a walnut occupied the right half of the pons and extended along the superior cerebellar peduncle of that side, reaching as far forward as the corpora quadrigemina. The gliomatous mass formed a prominence on the corresponding half of the floor of the fourth ventricle, and obstructed the Sylvian aqueduct.

In some cases both sides of the pons are involved, and the overgrowth of neuroglia extends forwards into the cerebral crura and the cerebellar peduncles, and involves the corpora quadrigemina. In a few it extends downwards into the medulla, and may even involve the cervical portion of the cord, as in a specimen described by Whipple.

Sometimes the gliomatous tissue is so abundant as to produce an enlargement of the pons and cerebral peduncles, as represented in Fig. 14.

The appearance of such brains is very peculiar : the basilar artery and its branches appear as though sunk in deep furrows, which cause the parts to resemble "a soft package tightly corded" (Dickinson). Such cases are rare, and in nearly all instances the patients have been under twelve years of age. Thus the case recorded by Percy

death the diseased parts are abnormally large, and on section exhibit a characteristic pale blue colour ; in thin sections the tissue has a delicate translucent appearance. The tumour itself is very soft, and imparts to the fingers a sensation like fluctuation. When the parts are immersed in alcohol the tissue becomes firm, opaque, and

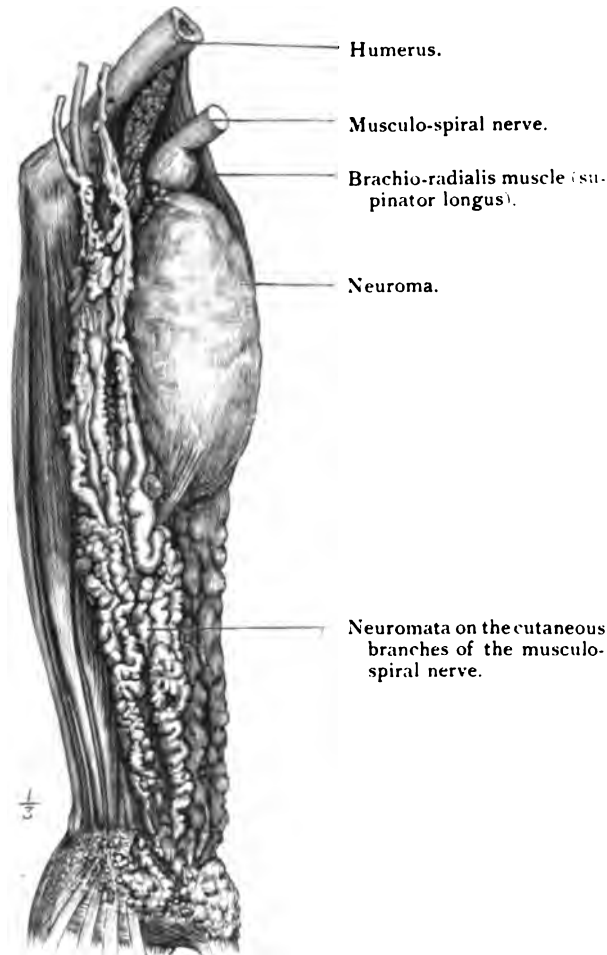


Fig. 13.—The arm represented in the preceding figure dissected; the musculo-spiral nerve and its branches are transformed into a plexiform neuroma (from Bland-Sutton's 'Tumours').

Kidd occurred in a girl six and a half years old. Gee's patient was a boy of nine years. In two cases described by Angel Money one was a boy of eleven years, and the other a girl of six and a half years. Goodhart has described a specimen from a boy aged nine years ; but Schultz has observed one in a man of thirty-two years. The relations of a glioma to the surrounding tissues are best seen in recent specimens. On examination soon after

white ; under these conditions it is particularly difficult to determine the limits of the tumour.

GLIOMATA OF THE SPINAL CORD.—A glioma in the spinal cord is a very rare tumour, and judging from the scanty records it would appear that a glioma in the brain is twenty times more frequent than in the cord. The tumour is imperfectly demarcated from the nervous tissue, and often causes a general enlargement of the cord, pro-

ducing an effect upon it like the gliomatous disease of the pons, crura, and medulla shown in Fig. 14.

GLIOMA (so called) OF THE RETINA AND OPTIC NERVE TUMOURS.—The consideration of neuro-fibromatosis would not be complete without some reference to glioma of the retina. This disease is very closely akin to the affection we have been considering in that it occurs in infancy and early childhood, the common period being the first four years of life, and it is almost unknown after the twelfth year; also the peculiar way in which it recurs after removal, the rarity with which it dis-

portance, because in some reported cases sarcomata have been traced, especially in children, to the pial sheath of the nerve, and in structure are closely allied to retinal glioma. The malignancy of optic-nerve sarcoma, though pronounced, is not excessive.

It is a valuable result of the great labour expended by many observers that six apparently diverse conditions, such as neuromata, ganglionic neuroma, plexiform neuroma, molluscum fibrosum in its solitary as well as its pendulous and nodular forms, sarcoma of nerve, and glioma of the central

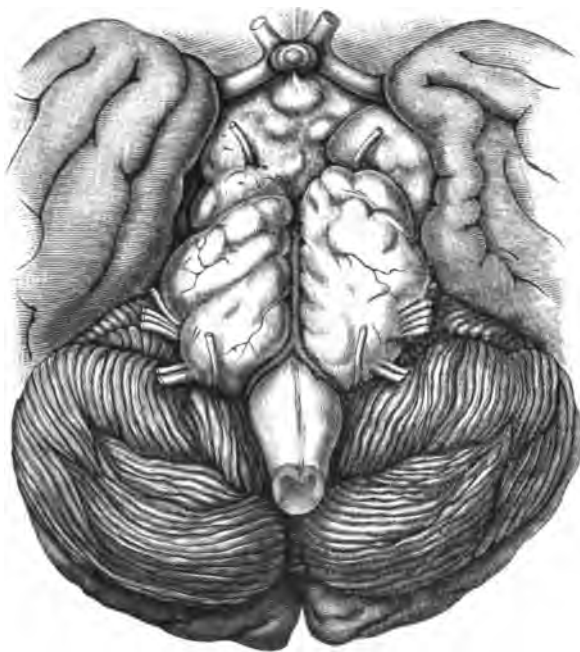


Fig. 14.—Bilateral gliomatous enlargement of the pons and crura cerebri (Angel Money).

seminates, and its liability to attack both eyes indicate that a congenital defect underlies this condition.

The optic nerve.—This is a complex structure, and in the embryo is preceded by an outgrowth from the brain known as the optic stalk; this is hollow, and consists of epithelial cells. This stalk is ultimately replaced by a nerve, the nerve elements of which are in part derived from the retina, and in part perhaps from the brain (Robinson). The early tissue of the optic stalk is identical in structure and continuous with the sustentacular tissue of the embryonic retina. These facts are of im-

nportance, because in some reported cases sarcomata have been traced, especially in children, to the pial sheath of the nerve, and in structure are closely allied to retinal glioma. The malignancy of optic-nerve sarcoma, though pronounced, is not excessive.

It is a valuable result of the great labour expended by many observers that six apparently diverse conditions, such as neuromata, ganglionic neuroma, plexiform neuroma, molluscum fibrosum in its solitary as well as its pendulous and nodular forms, sarcoma of nerve, and glioma of the central

nervous system and its prolongations are all due to a common cause, overgrowth of the delicate connective tissue which immediately surrounds the true nerve elements, the manifestation depending upon the particular part of the nervous system affected, the brain, spinal cord, large nerve-trunks, terminal nerves in the skin, or the delicate sustentacular framework of the retina. To my mind this result may be ranked with some of the greatest achievements of pathological histology, and it will serve as a reprimand to some who are disposed to regard this branch of inquiry as "having had its day."

A CASE OF ENTERECTOMY FOR FÆCAL FISTULA.

BY

F. C. WALLIS, B.A., M.B., F.R.C.S.,

Assistant Surgeon to Charing Cross Hospital and to
St. Mark's Hospital.

Z. G—, aged thirty, came under my care for this operation on November 1st, 1897.

Her previous history is briefly as follows :

On March 13th, 1897, a double pyosalpinx was removed, and during the operation a piece of intestine, which was adherent to the right tube, on being separated was denuded of its peritoneum.

On March 16th the abdomen was opened on account of acute intestinal obstruction, which was caused by the piece of damaged intestine becoming adherent to the pedicle, and getting kinked. The gut was brought up to the skin, and an artificial anus established.

1. The edges of the fistula were drawn together by a suture, and the whole scar was shut off by a collodion scab. This was done to prevent the fluid contents of the intestine running out on to the operation area.

2. An incision about four inches long was made parallel to, and about one inch to the right of, the former scar. Skin and muscles were cut through, and all bleeding stopped. The peritoneum was then incised.

On examining the intestine where it was attached to the skin, it was obvious that resection would have to be resorted to, as the calibre of the gut at this part was much diminished. The bowel was adherent for about two inches, and on drawing it out of the wound two coils of intestine were found attached by a band ; this was divided.

The loop of bowel was now clamped in six places, A1, A2, B1, B2, C1, C2, the contents having been first squeezed beyond A1, A2 (Fig. 1).

The skin incision was next completed by making another cut through to the left of the fistula, and

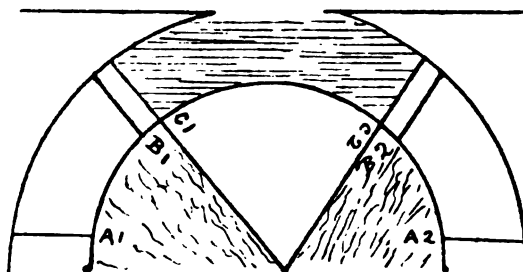


Fig. 1.

On May 12th, 1897, she first came under my care, when I endeavoured by a plastic operation to close the opening. This attempt failed completely, and the failure at the time was attributed to the persistent vomiting which the patient suffered from after the anæsthetic.

July 27th another attempt was made under *eucaine* to close the opening, but this was no more successful than the last.

On November 1st she returned to the hospital with the opening somewhat smaller, but a great deal of liquid fæcal matter flowed from the wound, and the surrounding skin was raw and tender. The patient's general health was good.

November 9th the following operation was performed :

joining at each end with the first incision. The elliptical piece of skin, with the underlying tissues, and the adherent gut were then pulled gently out of the wound, and the gut was divided between B1 and C1, and B2 and C2. The whole of the piece of intestine between C1 and C2, with the skin and abdominal tissues, was removed, and with them a V-shaped piece of mesentery. The cut edges of the mesentery were tied together in a bunch, and no time was wasted in ligaturing each individual vessel. A special ligature was applied at B1, B2, close to the mesenteric border, bringing the edges close together, and a Bailey's decalcified bone tube (Figs. 2 and 3) was inserted into each end of the cut intestine.

Two stout ligatures were now passed round the

intestine, tying it quite firmly to the tube at the two grooves (thus obviating any leakage as long as the stability of the tube lasted). The free ends of the bowel were cut close to the ligatures, and the intestine was then reduplicated, as seen at β (Fig. 4), all the way round the tube, the approximated serous surfaces being united by five or six Lembert sutures, especial care being paid to the mesenteric border. The exposed intestine was washed with warm saline solution, and returned to the abdomen, the peritoneum closed by a continuous suture,

quite normal in character. The temperature had never risen to 100° since the operation, and the patient was as well as one could wish her to be. The stitches were removed on the tenth day, when some brown semi-purulent fluid escaped from around the skin edge, and there was a small collection of fluid under the skin. This was swabbed out, and packed with iodoform paste and gauze, and soon granulated up.

The patient's recovery from now on was uninterrupted, and she left the hospital on December

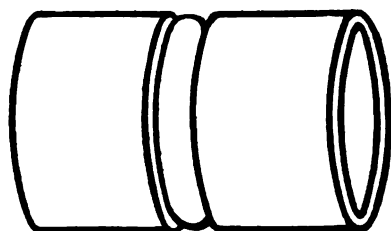


Fig. 2.

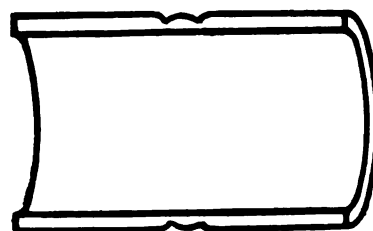


Fig. 3.—Section through tube.

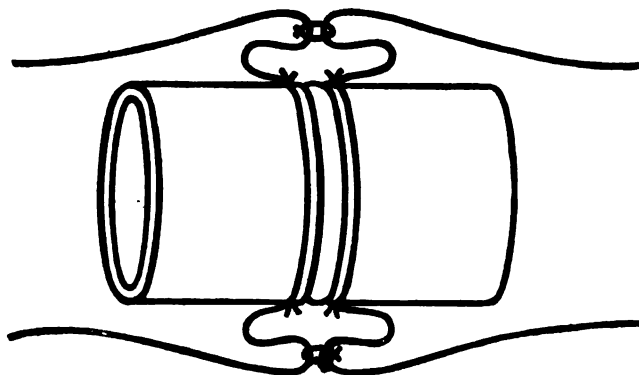
 β

Fig. 4.

and the muscles and skin united separately with interrupted sutures.

The operation lasted one and a half hours, and the patient bore it very well indeed.

The subsequent history of the patient is that for thirty-six hours there was considerable retching and vomiting, but at the end of this period it ceased entirely. She was nourished at first by enemata only, but on the third day small quantities of fluid were given by the mouth, this amount being gradually increased. The bowels acted on the fifth day after the operation, the action being

quite well.

It is just over two years since this patient left the hospital, and she is in the best of health, and she has been so ever since. I saw her last in October, 1899, when the abdominal wound was indicated by a white scar, which showed no tendency to yield.

Remarks.—As this is, I believe, the first published case in which Bailey's decalcified bone tube has been used, it may be desirable to draw attention more particularly to one or two details in connection with it.

Before doing this, however, I should like to add a little to the notes with regard to the two unsuccessful attempts made to close the intestine before resection was resorted to.

In both instances the idea was the same, viz. to endeavour to strip back the parietal peritoneum without disturbing the existing adhesion between it and the intestine, and then to oversew the opening by a series of Lembert's sutures. This is a method which was advocated by the late Mr. Greig-Smith, and which can be carried out in nearly every instance when the *large* intestine has to be dealt with. But in the case of small intestine the matter is a much more difficult one, besides which the result, even if temporarily successful, may lead to a stricture eventually. This it was evident would have been the result in the present instance.

Another matter of importance is the condition of the skin. The action of the fluid contents of the small intestine on the skin is one of digestion, and in a short time the area all round the fæcal fistula becomes inflamed, raw, granular, and is exquisitely tender.

Such being the case, it is quite hopeless trying to get the skin clean, but its condition can be much improved by getting the patient to lie in the prone position always, and allowing the fæcal discharge to pass directly into some receptacle, and thus avoiding its contact with the skin.

When the wound edges are being brought together at the end of the operation, the sutures should *not* be passed through the skin proper. All the remaining tissues are brought together, but the skin edges are only apposed by the approximation of the other tissues. By this means any accumulation of serum under the skin is avoided, and such delay as was caused in the case above is obviated.

To return to the tube. The advantage rightly claimed for it is that the decalcified bone lasts sufficiently long as a protective internal support to enable the reduplicated intestine, which has been united all round the tube, to heal completely before the intestinal contents come into direct contact with it, viz. the intestine, and thus any chance of leakage is done away with.

As it is now over two years ago since the operation, it may fairly be said that any question of stricture occurring may be discounted.

That the decalcified bone tube *does* act as a protective support for many days is proved by the following melancholy case :

Three and a half years ago I performed inguinal colotomy on a man at Charing Cross Hospital for intestinal obstruction. About eighteen months later, as we had reason to believe that the obstruction no longer existed, I resected the bowel at the colotomy wound, and inserted one of Bailey's tubes. All went well until the eighth day, when the man died in a few hours from evident perforation. Post-mortem showed that the bowel had perforated well above the tube, but *the part of the tube itself with the sewn gut around it was still intact.*

I should doubt whether the decalcified bone would remain as long intact in the small intestine, on account of the stronger digestive properties of the secretions here, but there is no doubt that for resection of the small intestine it is a great help, and is safe and simple ; and when compared with end-to-end suture much time is saved.

I cannot recommend it for resection of large intestine because the thickness of the bowel makes the whole operation a more difficult one. Simple suturing seems here to be safer.

The Surgical Treatment of Appendicitis.

—Clubbe ('Australasian Medical Gazette,' Dec. 20th, 1899) notes that in the Prince Alfred and Sydney Hospitals, during the last six years, there have been 347 cases of appendicitis, with fifty-seven deaths—a mortality of 16·1 per cent.

The appendix was removed in the interval between the attacks fifty-nine times, with six deaths ; thirty-five cases of abscesses gave eight deaths ; forty-five cases of general septic peritonitis gave forty-three deaths. In twenty-nine of these cases the abdomen was opened, the appendix removed, and the peritoneal cavity flushed out and drained, but only two recovered. —*Therapeutic Gazette*, May 15th, 1900.

For Intestinal Dyspepsia with Flatulence.

R Pepsini puræ,
Pancreatini, āā ... gr. xxx
Pulv. carbo. ligni
Bismuthi subgallatis, āā ... gr. lx
M. Ft. powders xii. Sig. One before each meal. —WELLS, *Medical Record*.

Tubal Abortion.--This means the outpouring of blood throughout the abdominal ostium, together with the formation of a mole. The latter may be retained within the tube, or expelled with the blood into the peritoneal cavity, and the tubal abortion is accordingly described as complete or incomplete. Complete tubal abortion is accompanied by hæmorrhage, which is usually severe, but is not repeated, and may not be fatal; but with incomplete abortion the tendency to bleeding continues as long as the mole is retained, just as a retained placenta leads to continued uterine hæmorrhage. The blood may be poured out abundantly, or it may assume the character of a "blood-drip," as Taylor calls it. The effused blood is called a hæmatocele; this term was formerly used to describe a definite pathological condition, whose origin was not known. Now it is almost universally regarded as due in every case to tubal pregnancy, and as descriptive of a separate condition the term may be regarded as obsolete. Hæmatoceles vary in character; when due to tubal abortion the blood is generally circumscribed so as to form a definite tumour. On the other hand, if caused by tubal rupture the limiting membrane may be slender and ill-defined, and liable to sudden and marked alterations from fresh bleeding; or the escape of blood may not be circumscribed, but "diffuse," when it is checked only by operation or death. It does not then come properly under the category of a hæmatocele. From this description the nature of the symptoms of a tubal abortion may be inferred. The patient is first seized with a sudden faintness, accompanied, as a rule, by sharp pain; this, if the bleeding be free, merges into a deepening collapse. When the latter takes the form of a blood-drip the patient may partially recover, although liable to recurring attacks of collapse when the retained mole leads to repeated outpourings of blood. Sometimes each attack is accompanied by sharp pain, due to "tubal colic," and in some of these cases it is found, on opening the abdomen, that the tube has repeatedly filled with blood, which has become converted into a clot forming a cast of the tube, and that each cast has been expelled with a fresh accession of pain and bleeding. On vaginal examination of a case of tubal abortion a boggy tumour is found occupying the pouch of Douglas, and on one or other side a swelling is

felt in the situation of the tube. The lateral swelling is more marked in cases of incomplete tubal abortion. The diagnosis is completed by the history of the case, which elicits the fact that the patient had missed one or two menstrual periods, and thought herself pregnant; she may state that a relatively long period of sterility has elapsed since her last pregnancy, or that she has not been previously pregnant.

Later Tubal Rupture.--This, like tubal abortion, generally occurs between the eighth and twelfth weeks of pregnancy—unlike the early rupture, which generally takes place into the peritoneal cavity or into the broad ligament, and the symptoms will vary accordingly. In the former case the symptoms are sudden and alarming; in the latter case they are less marked, and may be followed by complete recovery. The history of the case resembles that described under tubal abortion. The condition found on vaginal examination, when the rupture is intra-peritoneal, is hardly distinguishable from that found in the case of tubal abortion, but when the rupture is mesometric there is no fulness to be discovered in the pouch of Douglas; on the other hand, the lateral swelling is much more marked. A large proportion of the cases formerly described as pelvic hæmatoma belong to this category; it is the only kind of tubal pregnancy that can be safely left without surgical interference under proper observation, but even these cases may require operation sooner or later.

THE two preceeding paragraphs are taken from the eighth edition of the **PRACTICAL MANUAL OF DISEASES OF WOMEN AND UTERINE THERAPEUTICS** for Students and Practitioners, by H. MACNAUGHTON-JONES, M.D., M.Ch., just published by Messrs. Baillière, Tindall & Cox. The great success this work has attained is emphasised by the appearance of the eighth edition, and little need be said about the merit of a volume which has earned for its author such a well-deserved reputation. Some idea of the size of the book may be gathered from the fact that the number of pages amounts to 947, with 640 illustrations and 28 plates. As might be expected, the progress that has been made in the subjects treated has been so considerable that much new material has had to be added, and the technique of the modern methods of operating has been very clearly set forth. The eighth edition well maintains the excellence of the earlier issues, and furnishes medical men with a standard work on diseases of women, of use not only in daily work, but also in those difficult and doubtful cases requiring more than usual care and thought.

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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A CLINICAL LECTURE

ON

AORTIC VALVULAR DISEASE.

Delivered at the Hospital for Consumption and Diseases of the Chest, Brompton, by

SIDNEY MARTIN, B.Sc., M.D., F.R.C.P., F.R.S.,

Assistant Physician to the Hospital and Physician to University College Hospital.

GENTLEMEN,—I think we can very profitably spend the time at our disposal in discussing some of the anatomical points in relation to aortic valvular disease, as well as the clinical symptoms associated with such disease, and the relation to disease of the other valves of the heart. Acute aortic valvular disease results chiefly from rheumatic fever and scarlet fever, and is also associated with chorea, and with Bright's disease. One does not quite know whether the acute disease of the valves in Bright's disease is of the same nature as that of rheumatic fever or scarlet fever. At any rate it is a fact that you may get vegetations on the valves, acute endocarditis, in Bright's disease as well as in rheumatic fever. Among the acute forms may be mentioned infective endocarditis, which is a special infection due to micro-organisms of one or other kind; it may be either primary, or it may occur in the course of pyæmia. The valves are primarily affected by the organisms, or the organisms may infect a chronic lesion of the valves.

The chronic forms of aortic valvular disease are of greater importance, inasmuch as these cases last a good many years, and the prognosis has to be made in many instances. The two chief causes of chronic disease of the valves are the chronic inflammation resulting from acute endocarditis and atheroma commencing in the aorta and spreading to the valves, in some cases, no doubt, associated with syphilis. It is extremely difficult in many instances to state any definite relation between syphilitic disease and aortic valvular disease. But *post mortem* one finds specimens which are

not ordinary specimens of atheroma, and which are not ordinary specimens of chronic induration following acute inflammation. These occur in syphilitic subjects, and anatomically are associated with great thickening of the vessels in the valve and beneath the valve; such thickening, and the general endocarditis and peri-arteritis, are extremely suggestive of syphilis. Some, indeed, maintain that syphilis is frequently the cause of aortic valvular disease. The chief result of the chronic disease may be twofold in nature: you may, on the one hand, have simply a thickening of the valves, with some degree of adhesion, which results in a narrowing of the orifice of the valves, and so there is produced an aortic valvular stenosis. Or there may be more or less destruction of the valve, so that the valves do not meet at the end of the ventricular systole, and there is then regurgitation into the left ventricle. Of course these two conditions may have extremes; there may be either extreme stenosis or extreme regurgitation, where the small flaps of the valves are left so as to be quite inefficient for closing the orifice at all, so that the greater part of the blood sent from the left ventricle into the aorta regurgitates into the ventricle. But there are all gradations between the extreme stenosis and extreme regurgitation, from simple slight stiffening of the valves, producing some degree of stenosis with regurgitation, to slight irregularity in one valve, which makes but little difference to the circulation of the blood. In all cases of valvular disease of the heart, which is complicated by disease of the other valves, one has to gauge, in any particular case, the lesion which is most pronounced, from the point of view of prognosis, and even of treatment. Well-marked aortic stenosis is a very different disease from well-marked regurgitation. In well-marked stenosis the chief point as regards its pathology is that there is a difficulty in the entrance of the blood into the aorta. This difficulty leads to great hypertrophy of the left ventricle. Both experimentally and clinically it has been noticed that there may be a moderate degree of stenosis of the aortic valve, uncomplicated, as in the case when the disease is due to atheroma, and there may be no symptoms, no change in the circulation of the blood. Cohnheim experimentally tied a ligature round the aorta near the valves, and, taking at the same time the blood-

pressure in the carotid artery or in the femoral artery and the venous pressure in the jugular vein, he found that if the ligature were slowly tightened, and to some appreciable extent, the blood-pressure after a time did not alter, and the venous pressure did not alter. That means that the heart contracts more forcibly, so as to drive the blood through the narrowed orifice. Exactly the same occurs in disease. If there is a moderate degree of stenosis of the aortic valves the heart hypertrophies, so as to drive sufficient blood or the normal blood with sufficient force through the aorta to keep up the needs of the circulation. Therefore I say that in many cases of aortic stenosis the patient has no symptoms—there is no dyspnoea, nor anything which points to disease of the valves. It is remarkable that some of these patients may go on for years, and come for treatment only when the stenosis is very pronounced, and yet there has been no symptom of the disease during a long period. When there is extreme stenosis, so that the heart by hypertrophying or acting more strongly is not capable of overcoming the obstruction—not capable of discharging the normal amount of blood into the aorta,—then you have, as in most valvular lesions of the heart, a fall in the arterial blood-pressure and a soft pulse results. At the same time there is apt to arise an increase in the venous pressure, and its results, such as congestion in various parts of the body. In disease the most marked cases of aortic stenosis occur, not as a result of rheumatic fever, but as a result of atheroma. I show you a very good specimen illustrating stenosis of the valves, in which the valves are greatly thickened and calcareous, in which their edges are joined, leaving only a narrowed irregular chink through which the blood can go. Associated with this there is great hypertrophy of the left ventricle, and also enlargement of the right ventricle. In aortic regurgitation the hypertrophy takes place as in aortic obstruction, but dilatation is frequently more evident than hypertrophy. Aortic regurgitation is a disease in which there are earlier symptoms and more marked symptoms than are present in aortic stenosis. The ventricle tends to be more overfull than in aortic stenosis, and so you get more rapid dilatation. You will see here specimens of aortic regurgitation, which may be taken as typical specimens of regurgitation as seen after death.

The most obvious point about the heart is its very large size, an enlargement due chiefly to enlargement of the left ventricle, an increase in the amount of muscular substance, but an enormous dilatation of the cavity. If you look at the aortic valves you will see that they do not fit; they are irregular, as if a portion is planed off from the free edge. Of course during life in this case there was well-marked regurgitation. Here is another example of aortic regurgitation, in which there is a smaller left ventricle, the dilatation not being so great, although there is some degree of hypertrophy.

Aortic valvular disease shows, from a clinical point of view, most important associations with two other conditions, namely, aneurysm and mitral disease. The association of aortic valvular disease with aneurysm results, as a rule, from the atheroma which is associated with the aneurysm of the first part, or with the transverse part of the aorta, and the disease is the simple extension of the atheroma to the valves of the heart. In aneurysm of any part of the aortic trunk there is no hypertrophy of the heart. Though I believe the statement is made in some text-books that aneurysm of the aorta does lead to hypertrophy of the heart, it is not a fact; indeed, it is a physical impossibility. The mere presence of dilatation in the course of the aorta does not impede the circulation at all, there being no obstruction to the flow of blood, simply due to a saccular dilatation in the course of the artery. There is hypertrophy in the left ventricle in aneurysm only when it is associated with aortic valvular disease, or with chronic renal disease (granular contracted kidney), in which there is disease of the peripheral arteries, increasing the resistance to the circulation of the blood.

The association of mitral disease is more important from a clinical point of view. You may consider that aortic valvular disease is associated with mitral disease in two aspects. Aortic valvular disease leads frequently to mitral regurgitation, simply from the fact that the regurgitation of blood from the aorta into the left ventricle is aortic regurgitation, and the accumulation of blood from the resistance to the exit of blood from the ventricle in aortic stenosis tends to make the left ventricle overfull. This over-filling leads in many instances, indeed in most instances, to dilatation, and may lead to great dilatation when no hypertrophy

occurs. The great dilatation of the left ventricle resulting from either of these two conditions dilates the mitral valve if it is normal, that is to say, if it has no chronic disease. If it has a chronic disease of its own it cannot dilate; when it dilates there is mitral regurgitation, and you get the familiar sequence of events which you observe during life. In a patient who has the typical signs of aortic regurgitation, with hypertrophy of the heart and the basic murmur, and who subsequently develops the mitral regurgitant systolic murmur at the apex conducted into the axilla, that is mitral regurgitation, the direct consequence usually of aortic regurgitation.

But you have mitral disease associated with aortic valvular disease in another aspect, that is, when there is actually organic disease of the valve. This organic disease is usually the result of damage to the valve by the rheumatic fever or the scarlet fever at the same time as the aortic valves are damaged, or it may be the result of atheroma. Here are some good specimens illustrating this condition. The left ventricle is open, and it is somewhat dilated, and perhaps somewhat hypertrophied. The aortic valves are thickened and stiffer than normal, and partly united, resulting in regurgitation through the valve as well as some stenosis. The mitral valve looked at from the top is a mere slit; it is greatly thickened at its edges, and it is one example of the button-hole mitral. On looking at it below you see a hypertrophy of the muscoli papillares and the shortened and thickened chordæ tendineæ. Here is another specimen illustrating the same characters, with, however, more disease of the aortic valves. But the ventricle contrasts as regards size with this other specimen, where there is enormous dilatation of the ventricle associated with hypertrophy. Here is a more recent specimen, which I shall refer to again from a clinical point of view, which shows the same thing. It is rather a larger heart than the other two, and there is well-marked mitral stenosis, as well as some degree of aortic valvular disease—great enlargement of the left ventricle due to hypertrophy and dilatation and enlargement of the right side of the heart. In well-marked mitral stenosis associated with aortic valvular disease you do not get great enlargement of the left ventricle; you get a small heart with some dilatation of the right ventricle, perhaps some hypertrophy; but the

left ventricle does not enlarge, because it does not get enough blood to cause it to do so. If the mitral orifice is stenosed blood passes with difficulty from the left auricle into the left ventricle, and the consequence is that the left ventricle is badly supplied with blood, therefore it does not dilate. The cavities of the heart cannot dilate unless they get a sufficiency of blood. If the mitral stenosis is only slight, then you may get a very large left ventricle in aortic valvular disease.

Now as regards the symptoms and clinical aspects of these two chief conditions—aortic stenosis and aortic regurgitation. Aortic stenosis, well-marked and existing by itself, is a very rare disease. Of pure aortic stenosis I only remember to have seen two cases within the last five years; I mean cases where almost the sole condition was one of great constriction of the aortic orifice. In all cases of well-marked aortic stenosis there is great hypertrophy of the left ventricle, giving rise to a very heaving impulse, one to one and a half inches outside the normal position of the heart's apex-beat, four to five inches from the mid-sternal line, and in the fifth and sixth interspaces. As a rule, at any rate if the patient is at all in good condition, not seriously ill, there is no epigastric pulsation. That is, an enlarged right side of the heart is absent in the cases of aortic stenosis unless there are complications. Of course there are great variations in the amount of hypertrophy and dilatation of the heart.

As regards murmurs there are two or three different conditions which are of great importance. A certain degree of aortic stenosis, sufficient to produce a murmur, is usually associated with aortic regurgitation, and everybody is familiar with the to-and-fro basal murmur, like a rubbing sound, which occurs along the sternum in cases of obstruction and regurgitation. In other cases, especially in men at or past middle age in whom atheroma has begun, there is not infrequently a localised basic systolic murmur heard between the third rib cartilages, which may or may not be associated with obvious hypertrophy; that is the earliest sign of the stiffening of the valves. This may go on till you get the characteristic auscultatory signs of advanced aortic stenosis, which when once experienced can never be mistaken. There is a great hypertrophy of the left ventricle; there is a localised heaving impulse; there is no epigastric

pulsation, and no signs of dilatation of the right side of the heart. All over the cardiac area you hear a loud and rough systolic bruit, which is most distinct, and almost rasping, at the aortic cartilage or between the third rib cartilages.

The chief auscultatory sign in such a condition relates to the second sound; it is almost absent. The aortic second sound is quite absent, and what there is of the second sound is from the pulmonary valves. The second sound heard at the heart's apex is produced solely by the closure of the aortic valves, and in these cases the second sound is quite absent at the apex, and may be absent at the sternum and at the aortic cartilage, though you may hear a faint second sound over the pulmonary cartilage. This absence of the second sound means such a great constriction of the valves, and such a stiffening of the valves, that they cannot close after the blood has passed; and very often in these cases the valve is a chink surrounded by the thickened and adherent cusps.

I lay stress on the absence of the second sound here, because it is a great element in the prognosis. If there is silence after a rasping murmur, that means it is a more advanced case, and the prognosis is not good. Sometimes they die suddenly, and sometimes they die from giving way of the right side of the heart.

Some patients with aortic stenosis may go for many years without complaining of any symptoms. An instance of that is the case of a gentleman who lived a hard literary life, and took much exercise (bicycling), and yet he was forty-seven when first seen, and then had been having difficulty with breathing for only a few months, which first came on after a longish bicycle ride. There was enormous hypertrophy of the left ventricle, with a heaving impulse in the fifth and sixth spaces, and there was a loud rasping murmur at the base heard all over the cardiac area. The second sound was inaudible, and the case was diagnosed as advanced aortic stenosis. He went on taking much exercise, in spite of advice, and developed cyanosis, which showed that the right side of the heart was giving way. There was increased tension in the venous system, and he died in three weeks from œdema of the lungs, owing to failure of the right side of the heart. He must have had this disease for many years. Another instance may be quoted of a woman aged fifty-three, in whom the

cardiac disease originated in rheumatic fever, the last attack of which was at the age of fifteen years. She had led a fairly active life, and when seen had well-marked aortic stenosis, with great diminution in intensity of the second sound.

The symptoms which patients complain of are chiefly dyspnoea and palpitation. Dyspnoea is almost constantly the first symptom they have, and sometimes palpitation. They may complain of anginal or pseudo-anginal attacks.

The patient with well-marked aortic stenosis has not the pallor of the patient with aortic regurgitation, and the pulse contrasts also with aortic regurgitation, inasmuch as it is almost always small, and generally slow, with none of the characteristics of the "water-hammer" pulse.

As regards the symptoms in aortic regurgitation, the patients come complaining of the same symptoms as in well-marked aortic obstruction, but they also complain of syncopal attacks. Palpitation, the onset of oedema of the lungs, and dyspnoea are more common in regurgitation than in obstruction. Both forms show great enlargement of the left ventricle, and in the early stages there are no signs of enlargement of the right ventricle.

The murmur in aortic regurgitation is one of great interest. The typical murmur is diastolic, heard at the base between the third rib cartilages, conducted downwards to the sternum and to the apex; it is also conducted upwards towards the sterno-clavicular joint, but is not conducted into the axilla. If these cases always showed these signs they would be diagnosed with the greatest ease. The diastolic murmur in aortic regurgitation may be heard only over an area near the ensiform cartilage, to the left of the sternum; it is to the left of the sternum, from the fourth to the sixth ribs, and very slightly towards the apex, in the position in which the diastolic murmur of mitral disease is frequently localised. It is sometimes the fact that with the greatest care the diastolic murmur of aortic regurgitation is mistaken for that of mitral disease, and *vice versa*. I will mention the points presently which will enable you to distinguish between the two. Occasionally the diastolic murmur of aortic regurgitation is heard best at the apex. Like the diastolic murmur of mitral disease, it may disappear when the patient rests in bed; so it not infrequently happens that

you may examine a man in the out-patient room and find a distinct murmur, and yet when he is admitted and lying in bed the murmur has disappeared. You can bring out the murmur by letting the patient walk up and down the ward.

It is easy to diagnose aortic regurgitation if the murmur is in the typical place, heard at the base, down the sternum, and towards the apex; but when it is loudest at the left of the sternum, then the diagnosis from mitral murmur becomes difficult.

The diastolic murmur of mitral disease has a different place in the cardiac cycle to the diastolic murmur of aortic regurgitation. In aortic regurgitation it occupies either the whole of the second sound or part of it. As a rule with mitral stenosis the murmur appears to be out of gear with the cardiac cycle; the murmur further appears quite independent of the sounds produced in the cardiac cycle. That is one point, but it is a very difficult thing to express in words; in suitable cases it is easily demonstrable.

Another point, which is more important in the diagnosis, is that the diastolic murmur of aortic regurgitation does not alter its time; it is always diastolic, and always occupies one or other part of the second sound.

The typical pre-systolic murmur is a long murmur going up to the first sound, and is really produced by the slow contraction of the auricle, *i.e.* before the contraction of the ventricle, before the first sound. The diastolic murmur of mitral stenosis occurs when the heart is showing want of compensation, and the explanation—the only one possible—is that instead of the auricle contracting just before the ventricle, as it ought to do, it contracts at another part of the diastole. That is, the contraction of the auricle is thrown out of gear in the cardiac cycle, because the pressure of blood is enormously increased, so that it cannot cope with it.

I think that the proof of this last explanation lies in the fact that when you give such a patient digitalis to make the heart less frequent and more regular, to increase the compensation, this diastolic murmur disappears and comes before the systole, and then the heart becomes regular.

The last point I wish to refer to is the treatment of these cases. Of course very few people nowadays, I hope, would give digitalis in advanced cases of aortic regurgitation or aortic obstruction. Such

disastrous results may happen from its use that in these cases digitalis is distinctly contra-indicated.

The question of treatment in aortic obstruction is not quite the same as in aortic regurgitation. In obstruction, by giving small doses of digitalis when the compensation is failing you may increase the power of the ventricle, and so actually help the patient. In advanced cases, if you give digitalis you overwork the heart, and you get syncope and increase of dyspnoea.

In well-marked aortic regurgitation, where you have the familiar changes in the pulse well marked, digitalis is contra-indicated, because it delays the blood in the ventricle, and so tends to increase the regurgitation.

In cases with mitral and aortic disease associated, we have to discuss whether digitalis ought to be given or not, and the indications for the administration of digitalis rest upon the relation between the degree of mitral disease and the aortic valvular disease. If there is evidence chiefly of mitral disease, then, as a rule, digitalis given in judicious quantities does good by diminishing the want of compensation. Such indication that the mitral disease is greater than the aortic disease is obtained from the pulse and general symptoms, as well as from the physical signs in the heart.

The chief indication for the administration of digitalis is from the pulse, because, even though we may have the typical physical signs of aortic regurgitation, yet there may be associated with that disease the rapid and very frequent pulse which is typical of mitral disease, or there may be a pulse not so frequent, about 90, and irregular. In these cases small doses of digitalis do a great deal of good. In many of the cases of mitral disease secondary to aortic regurgitation, digitalis does harm by increasing the dilatation of the ventricle, and strychnine and stimulating remedies are better.

'THE NEWER REMEDIES' is the title of a very useful little work for which great praise is due to Dr. Coblenz. The multiplicity of names, and the great number of new drugs prescribed by modern practitioners, no doubt originally suggested the issue of this book to the publishers, Blakiston's Son & Co., of Philadelphia. The volume supplies intelligently arranged information concerning useful drugs which are often required to be prescribed, but which are passed over and omitted in ordinary text-books.

TUBERCULAR DISEASE OF THE HIP-JOINT.

Delivered at Guy's Hospital, June 8th, 1900.

By **W. ARBUTHNOT LANE, M.S.**,
Surgeon to Guy's Hospital, and to the Hospital for Sick Children.

BEFORE considering tubercular disease of this joint I will remind you of several mechanical points in connection with it, to which I have called your attention on previous occasions.

POSITION OF JOINT IN RESTING SUPINE POSTURE.

—If this position be assumed, the thighs roll outwards around the long axis of the limb till the inner margins of the feet include between them an angle of about 90°. The size of this angle varies within comparatively wide limits with the age, vigour, and occupation of the individual. While in a child, with a lax condition of joints, this angle is much in excess of a right angle, in the middle-aged coalheaver it is perhaps correspondingly less than the average given here. This rotation of the limb, which till I pointed it out appears to have completely escaped the notice of the surgeon and the anatomist, whose minds are more apt to observe the minutest detail than to grasp the most conspicuous and simple facts, is due to the vast preponderance of the bulk of the thigh lying outside the vertical axis, around which the limb rotates upon the pelvis at the acetabular cavity: and it is controlled chiefly, if not almost entirely, by the resistance offered by the strong ligaments of the hip, which cover its anterior surface especially. This can be demonstrated beyond doubt by examining a body from about whose hip-joint the soft parts have been stripped.

RANGE OF FLEXION OF THE HIP-JOINT.—The range of flexion of the hip-joint is much more limited than the anatomist supposes, since he imagines that it is controlled only by the impact of the thigh against the abdomen. He has wrongly included in this the flexion of the sacro-iliac, lumbo-sacral, and lumbar joints, which occurs when the thigh is flexed upon the trunk till movement is resisted by the abdominal wall. Flexion of the hip-joint is limited by the impact of part of the neck of the femur against the margin of the ace-

tabular cavity. Roughly speaking, the angle of possible flexion is about 90° , but it varies widely with the age, vigour, and occupation of the individual in precisely the same manner as does the outward rotation of the thigh in the resting supine posture. What is of exceedingly great importance is that the degree of flexion is influenced in a most marked manner with associated adduction or abduction. Indeed, in extreme adduction the amount of possible flexion is very small indeed, owing to the early impact of the neck of the femur against the acetabular margin, while in extreme abduction the angle of possible flexion considerably exceeds a right angle. In other words, *the degree of possible flexion in the hip-joint varies inversely with the angle of adduction, and directly with the angle of abduction.* This fixation of the femur on the pelvis is of the greatest importance to the labourer in that it enables him to carry a very heavy load securely, and saves the enormous expenditure of energy which must take place if it did not exist.

When a heavy load is borne on the back, entailing some flexion of the trunk on the thighs, the degree of adduction of the pelvis on the femur is such that the hip-joint is continuously fixed alternately upon either femur. In consequence of this habitual impact of these bones on one another there is formed on the neck of the femur at this point a triangular articular area, the form and position of which varies with the particular character of the occupation followed by the labourer.

Fig. 1, which represents the anterior surface of the upper extremity of the femur of the coal-trimmer, shows this formation in the neck of the femur very well; while Fig. 2, which is a photograph of the posterior surface of the same bone, illustrates the remarkable changes which the hip-joint undergoes when a certain definite laborious occupation is followed.

When the hip-joint occupies its position of extreme flexion in any degree of abduction or adduction the femur represents a lever of the second order. Its fulcrum corresponds to the point of impact of the neck on the acetabular margin, its short arm to the head and portion of the neck above the seat of impact, while the long arm is formed by the whole length of the femur below this point. Again, if the leg be extended at the knee-joint, the long arm is formed by the

entire limb. You readily recognise that the force which can be brought to bear upon the neck, tending to break it, or exerted towards the displacement of the head from the cavity, is enormous.

In the young subject the weakest part of the neck of the femur is the epiphysial line. In the old subject the portion of the neck of the femur adjacent to the head becomes perhaps the least resistant part of the whole bone. The factors opposing displacement of the head of the femur are atmospheric pressure, the strain exerted by the muscles which surround the joint, and the resistance offered by the ligamentum teres and by the thin lower and back part of the capsule.

In the young and old these several resistances to displacement of the head of the femur exceed the breaking strain of the lever, while in the vigorous adult the reverse is the case, and dislocation instead of fracture results in response to force exerted through the leverage of the femur in the manner I have indicated.

The anatomist, after having formed many and various opinions on the subject of the ligamentum teres, all more or less opposed to one another, appears to have come to the conclusion that such an important and well-developed structure, to which he would grudge even the name of ligament, merely exists as a relic of some antecedent type, as you will find if you turn to page 361, line 24, 'Gray's Anatomy,' where you will read the following:—"The security of the (hip-) joint may be also provided for by the two bones being directly united through the ligamentum teres, but it is doubtful whether this *so-called ligament* can have much influence upon the mechanism of the joint." On the contrary, you will find that this ligament does exert a most important influence on the mechanism of the joint, and, as in any other structure in anatomy, its development varies directly with the demand made upon its functional activity in any particular case. This is demonstrated in the clearest manner possible by the study of the hip-joint in the various forms of labour.

Again, you can readily observe the marked increase in its thickness when an abnormal strain is thrown upon it, and it is made to take a greater share in transmitting weight than it is in the normal skeleton. In congenital dislocation of the hip-joint, which I believe results in foetal life through

the leverage action of the femur acting over a prolonged period upon the hip-joint, the capsular ligament is found to be considerably elongated and hypertrophied, and in such a way that it is strongest along the lines through which the greatest strain is transmitted. The ligamentum teres also is elongated in a corresponding manner, and is greatly hypertrophied and very useful in transmitting weight. I would point this out to those surgeons who perform various subcutaneous operations on the congenitally dislocated hip, and lay

THE POSITION OF REST OF THE HIP-JOINT IN THE ERECT POSTURE.—To determine the position of rest of a joint you must observe it when it is performing no function whatever. You will then find that its position is such that all the muscles which surround it are in a position of relaxation, and none of the ligaments sustain any strain.

The position of rest of the hip-joint in the erect position can be observed to perfection when the "easy erect posture" or the "asymmetrical position of rest of the trunk" is assumed, all the weight



Fig. 1.

stress on the division of the ligamentum teres. I must say I regard these operations and their results with the greatest suspicion. I believe that the particular character of the congenital dislocation is brought about by the degree of adduction or abduction associated with flexion in intra-uterine life. I do not propose considering the treatment of congenital dislocation of the hip which I adopt, as I have published it with my results very fully in previous papers, but I have merely alluded here to the condition in order to render the mechanics and functions of the hip-joint more clear for you.

of the body being transmitted through the hip-joint of the opposite side.

If you examine a child occupying this position, you observe that the thigh is flexed and abducted to a considerable extent upon the pelvis; and if you assume this posture and examine your several muscles about the joint, you will find them all soft and flaccid. If you carefully measure the angle of abduction and flexion, and place a dissected hip-joint in precisely the same position, you will observe that the ligaments about it are all relaxed.

POSITIONS ASSUMED BY INFLAMED JOINTS.—When a joint shows such evidence of inflammation as is manifested by an increase of its synovial fluid, or, in other words, when synovitis arises, it assumes the attitude of rest. It does this for the reason that in this particular position all the ligaments which surround it are loose, and the excess of fluid in its interior can be accommodated to the best advantage and with least pain or discomfort.

Later, especially if there be any pain on movement of the part, the joint is brought into a position in which movement is controlled by the impact of

struction of the head of the femur or of the acetabulum.

Later still the force which the muscles exert in retaining the bones fixed upon one another may, by reason of a leverage action, result in the dislocation of the joint, and in the progressive displacement of the articular surfaces.

POSITIONS USUALLY ASSUMED IN HIP-JOINT DISEASE.—This is precisely the sequence of events that usually takes place in hip-joint disease. Since the disease may be very painful from its onset, and unaccompanied by definite effusion, it does not

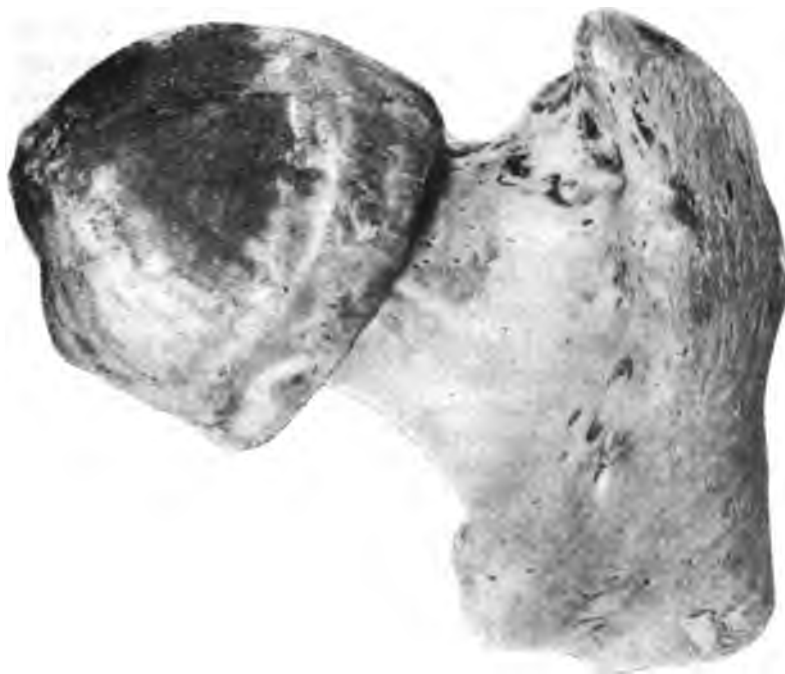


Fig. 2.

bony surfaces upon one another, if such a fixation is possible to the particular joint. In this position the bones are retained immovably on one another by all the muscles which are able to bring the joint into that relationship to one another.

To permit of a joint which has been flexed and abducted assuming such a position of flexion and adduction, it is necessary that the tension inside the capsule be reduced by the absorption of any excess of its contents or by its escape from the joint, or by the thinner posterior portion of the capsule becoming softened and stretched, or by some de-

follow that a patient, the subject of tubercular infection of this articulation, should necessarily assume these several postures in this order, but the joint may be fixed almost at once in a position of flexion and adduction. However, as a general rule the sequence of varying conditions of flexion is observed when the synovial membrane is affected. The synovitis which ensues causes the joint to assume its position of rest. The presence of material in the joint limits all movements somewhat, but owing to the free mobility of the pelvic and associated joints around a transverse axis, and to the short-

ness and obliquity of the fibres of the posterior ligament of the hip-joint, that of adduction when the knee is flexed is perhaps the most marked in the earliest stage. As, however, the posterior ligament is comparatively thin it soon yields, or in consequence of one of the other conditions already mentioned the forcible contraction of the adductor muscles is enabled to produce the next stage, when the hip is flexed and adducted, and still later dislocation ensues.

OBSTACLES TO EXTENSION OF JOINT.—The factors that oppose extension of the hip-joint in the early stage, in which the position of rest is assumed, are in the first instance the material within the capsule, whether inflamed synovial membrane or effusion, or both, and later the compensatory contraction of the flexor muscles and capsular ligament when the position has been occupied for a sufficient time.

In the position of adduction and flexion before the head of the bone has left the acetabulum, the acquired contraction of the flexor and adductor muscles and of the ligaments prevents reposition of the limb in the normal position of extension.

When the head has escaped from the acetabulum its adhesions, and its inability to pass upwards and forwards, together with the increased shortening of the muscles, etc., render it impossible to restore the axes of the femur and pelvis to their original mutual relationships. If any degree of flexion of the hip is present there must be a corresponding amount of over-extension of the sacro-iliac, lumbosacral, and sacral joints, varying in character with any abduction or adduction present, conditions clumsily grouped under the somewhat unmeaning and undescriptive term "lordosis."

THE MANNER IN WHICH THE HIP-JOINT BECOMES INFECTED WITH TUBERCLE.—As in tubercular infection generally, so in the hip-joint, the nidus which is provided by traumatism is very generally in evidence. This traumatism may, by bruising the articular surface, set up a synovitis which later becomes infected, or the layer of bone immediately beneath its hyaline covering may be injured and inflamed, or the line of growing cartilage and bone called the epiphysial line may be the seat of injury, and later the starting-point of tubercular disease.

There are several modes in which these component parts of the joint may be damaged.

Force may be exerted on the femur when

the hip-joint is flexed, or flexed and adducted, or flexed and abducted, when the growing line may sustain considerable damage, varying from a slight bruise of one portion of it to a complete separation of the epiphysis. How difficult it is to observe the frequency with which the last condition—namely, complete separation of the epiphysis—occurs as an antecedent in cases of tubercular disease is shown by the remarkable rarity with which this condition is recognised at the time of the injury in such subjects in whom the joint is not infected with tubercle later. Though I have seen and operated on a considerable number of cases of separation of the head of the femur, I have never seen one within three weeks of the injury, nor have I known one in whom the condition had been recognised at the time of the accident. In quite a large proportion of cases the patients made no mention of any injury to the parents, nor did they experience any pain. Arguing from these facts, I believe that complete separation of the epiphysis is by no means uncommon as an antecedent to tubercular infection. It is highly probable, however, that damage to the line, short of complete separation, is very much more frequent.

The articular cartilaginous covering of the subjacent bone and the epiphysial line are also liable to damage when the child falls on the side, the outer surface of the great trochanter striking the ground. The same parts may be damaged by force transmitted along the length of the bone when the child falls and alights upon its feet.

TREATMENT.—Our treatment must be based on certain definite principles.

GENERAL TREATMENT.—Not the least important part of it is the improvement of the vitality of the individual generally and of his several tissues.

An examination of the chests of these children generally shows a very poor respiratory capacity. A tape placed round the chest at the level of the nipples during normal respiration shows very little, if any, variation. Such respiration as there is is very superficial, rapid, and almost, if not entirely, diaphragmatic. On making the child inspire and expire to the utmost, the differences in the measurements will be found to be very much less than in the vigorous subject.

To improve the supply of oxygen the child should be made to perform daily such exercises as will ensure an increasing vital capacity.

It ought to be placed in the best air that the circumstances of the parents will permit, and the ventilation of the bed and other rooms occupied should be carefully attended to. By this means the vigour of the child and the performance of the functions of its several tissues are rapidly improved, and any tendency to pulmonary infection is very greatly reduced.

The teeth should be attended to, and the diet thoroughly overhauled and regulated.

MEDICINAL TREATMENT.—While the vitality and resisting power of the tissues is improved much benefit results from the administration of such germicides as are at our disposal. By their use we may inhibit largely the growth of tubercular organisms in a hip-joint, probably with nearly the same success that follows when they are used against the same disease after it has obtained a foothold in the lymphatic glands. The drugs I use are mercury, the iodides of ammonium, potassium, and sodium, with gradually increasing doses of liquor arsenicalis.

LOCAL TREATMENT.—Rest of the affected part is advantageous to the individual as opposed to the organism. The more perfect the immobility ensured, the greater is the capacity of the tissues attacked by the organism to stay the progress of the invasion, and later to destroy those bacilli that are already in possession.

If the joint is not retained in any modification of flexion it should be fixed securely in some apparatus of the character of a double Thomas's splint, which ensures more or less complete fixation of the joint, and permits of the patient being carried about with freedom from place to place.

If flexion associated with abduction or adduction are present double extension apparatus should be applied, traction being exerted on both sides symmetrically and in suitable directions, with the object of rendering the limbs parallel or as near so as possible, when the abnormal excess of material in the joint has been absorbed and the contracted muscles have relaxed. If there is any displacement of the head of the bone such extension is of little or no use, and operative measures should be adopted in the first instance. During the application of extension and when the thigh is fixed in an apparatus care should be taken to ensure the normal outward rotation of the limb, whose degree can be determined by that of the

leg of the opposite side. If anything this angle should be exaggerated on the diseased side, to meet any shortening owing to destruction of either or both of the opposing articular surfaces, and to resist the tendency to backward displacement of the head of the femur with the corresponding destruction of the acetabular margin which exists in these cases as far as possible.

Both general medical and local treatment should be carried on most thoroughly till all evidence of disease shall have disappeared. It is much safer to keep the patient an invalid for an excessive time than it is to attempt too early movement.

OPERATIVE TREATMENT.—More active surgical measures must be adopted in many cases, and with *two distinct objects in view*.

(1) In most cases the operation is performed with *the object of effecting a removal of tubercular products*, which should be as complete as possible; and the measures which enable the surgeon to perform this most thoroughly are those that should be adopted, providing always that they have no compensating disadvantages.

(2) In a certain proportion of cases the surgeon interferes solely with *the object of improving the patient purely from a mechanical standpoint*, as regards the possibilities of walking and sitting. In such cases the tubercular organisms have died and been absorbed, or are encapsuled in a latent condition. Such operations are performed for ankylosis of the hip-joint in any position, or for dislocation of the upper extremity of the bone on to the dorsum ilii.

(3) In a larger proportion than the last an operation is undertaken *with both objects in view*,—namely, the most complete possible eradication of the disease, and the most efficient possible restoration of the mechanics of the limb.

How best to effect the complete removal of tubercular material at a minimum risk to life, and with the least damage to the mechanics of the patient, is a very important problem, and the procedures adopted vary widely with the skill, intelligence, and experience of the surgeon.

I have tried the various methods that have been recommended, but have come to the conclusion that I can effect the most complete clearance of the joint through a transverse incision extending backwards along the upper margin of the tro-

chanter from the anterior inferior spinous process of the ilium. It is true that several large muscles are cut through in this way, and that often there is more loss of blood unless the surgeon is prompt in the use of his forceps, when there should be exceedingly little, since the wound is an open one, and bleeding points can be secured at once. The advantages which this method offers are—

1. By means of it the joint and the parts about it can be thoroughly exposed.



Fig. 3.

2. If there is any dorsal displacement the seats of disease in the femur and ilium can be got at readily.

3. If it seems advisable to make a new joint beneath the anterior inferior spinous process in front of the transverse axis of pelvic rotation in the manner I have advocated, and which affords the patient enormous mechanical advantages, the

head of the bone or the stump of the shaft or neck can be readily placed in position, and, if necessary, fixed there by a silver wire to the ilium through this incision.

The principle on which this operative procedure is based was first published in the 'Medico-Chirurgical Transactions,' 1888, in a paper entitled



Fig. 4.

"An Undescribed Method by which the Superjacent Weight of the Body is transmitted in Fracture of the Neck of the Femur, and the Bearing of the Principle involved on the Surgery of the Hip-joint;" and was amplified fully in a paper in THE CLINICAL JOURNAL, October 20th, 1897.

Figs. 3, 4, 5, 6, represent the appearance of two patients who have been operated on in this way.

Figs. 3 and 4 show a child, six years old, who was operated on in 1897 for extensive disease of both hip-joints, with several abscesses, which had burst on either side. The disease started when she was three and a half. When operated on, the thighs included an angle of 50° with the trunk, and

the formation of such a new joint, on one side only, in a child aged four and a half years, operated on in 1897. The disease had completely subsided, and the operation was undertaken only with a mechanical object.

The whole diseased area can then be thoroughly



Fig. 5.



Fig. 6.

the knees crossed. The dislocated upper end of the femur was on either side ankylosed by fibrous tissue to the ilium. A new joint was made beneath the anterior inferior spinous process in the usual manner on both sides. Caseous foci were present.

Figs. 5 and 6 represent the condition following

packed after the removal of the tubercular material, either for one or more days, with iodoform gauze to secure a nice dry surface free of organisms, when the edges of the muscles and skin can be accurately brought together. If it has been necessary to use sulphur because of the presence of other organisms,

or of considerable pocketing along muscle planes, the necessary subsequent packing can be carried out more effectually through this incision than in any other way I know.

Objection may be taken that a second or even a third anæsthetic may be required in the procedure I adopt, but that would apply equally to any form of operation in which the surgeon decides on packing the part, and so obtaining a thoroughly dry wound with a much more complete removal of the disease than could be effected by simply scraping, flushing, etc., at one sitting. Besides, it diminishes the risk of septic infection very greatly, and in consequence usually telescopes the duration of the convalescence considerably.

Although the muscles above the joint are freely divided the important anterior portion of the capsule escapes injury; and in early cases of disease of the joint, with perhaps little or no affection of the bones, this is subsequently of vital importance to the mechanics of the patient.

I will not venture to criticise other methods of operating, but simply insist that if it is decided to operate at all it is most important that the tubercular material should be removed in the most thorough manner possible. To make a hole into the capsule, to scrape about in the dark, however freely, and to irrigate as profusely as you wish, will not effectually clear such a cavity as the hip-joint of disease. It may be argued that in many cases such a partial clearance is sufficient, but to me it appears muddling, uncertain, wanting in thoroughness, and unscientific.

It would seem almost hopeless to reconcile the contradictory character of most of the accepted statements concerning the alkaloids of opium, but the confusion is explained by the fact that whenever the factor of impurity is likely to occur in any therapeutic agent then there are invariably to be found statements almost diametrically opposed to each other. With pure codein such as is issued in the tabloids which have been supplied for report by Messrs. Burroughs Wellcome & Co., the beneficial results claimed, of producing sleep without disturbance of the digestive tract and avoiding the feeling of sickness with its accompanying distressing headache, can be procured. In diabetes many prefer to prescribe codein, and its usefulness in chronic bronchitis has long been recognised.

WITH MR. HERBERT WATERHOUSE IN THE WARDS OF CHARING CROSS HOSPITAL.

LADIES AND GENTLEMEN,—To-day I wish to show you some cases of more or less surgical interest.

CASE 1. *Radical cure of a large irreducible umbilical hernia.* The first case is that of a married woman, aged fifty years. She was brought to me two months ago by Dr. M. M. Townsend, suffering from an irreducible umbilical hernia. Umbilical herniæ are exceedingly prone to be irreducible, because as the hernia protrudes at the umbilical orifice, or more frequently in adults just above or just below it, it pushes before it the great omentum. Therefore I think we always find omentum in the sac of the hernia. Then, of course, the omentum is followed by large intestine. Owing to the arrangement of the clothes in the female, we have a tendency for pressure to be exerted upon the hernia, in consequence of which we are apt to get the omentum contained in the hernia inflamed, and this leads to adhesions between the sac of the hernia and its contents. Omental adhesions are unquestionably more common in umbilical hernia than in any other kind of hernia. The reason that led me to operate in this case (which, of course, was not a favourable case by any means, because the woman is stout and flabby, and it was a very large hernia, absolutely irreducible) was the fact that she had had two very bad attacks of intestinal obstruction, very nearly necessitating operation. Slighter attacks had, she tells us, occurred fairly frequently, and one, therefore, made up one's mind that sooner or later, and in all probability sooner rather than later, it would be a question of operating for acute intestinal obstruction. Now we must bear in mind that when herniotomy is performed for strangulated umbilical hernia one finds that, instead of the bowels being in their normal condition, or better than in their normal condition by reason of their having been empty two or three days, we should have had the intestine bruised, or perhaps almost gangrenous, and the abdomen, instead of being fairly flat, would have been markedly distended and in many other ways the patient's condition,

owing to exhaustion from vomiting, etc., would be much more unfavourable. In this case there had been two attacks of intestinal obstruction of so marked a degree that for two days no wind passed, and the patient vomited very severely. Therefore Dr. Townsend and I felt we were abundantly justified in recommending the radical operation. That was done twenty days ago. The case has gone on uninterruptedly ever since. There was no real difficulty at the operation except that, the hernia having been in existence nine years, the adhesions were very dense, particularly round the neck of the sac, and the opening was a very big one. We, however, after some difficulty freed the omental adhesions and replaced the bowel, which was itself in two places adherent to the sac, and then separated and ligatured the neck of the sac, which was very much thickened; and having done that, closed the ring. The wound healed by first intention.

There is one point which is worth paying some attention to: that is that in suturing the linea alba in these cases it is difficult to obtain good firm union if you content yourself with suturing a round hole with fibrous margins. What you have to suture here is a somewhat lowly organised fibrous band. It is very much better to split longitudinally each half of the linea alba, opening thus the sheath of the rectus muscle, and then to suture separately (1) the posterior sheath of the rectus muscle, (2) the inner margins of the two recti abdominis, and (3) the anterior sheath of the same muscles. I do not say for a moment it is wise to operate upon all cases of umbilical hernia, especially in elderly stout women; still I think it is the path of wisdom where you know there is an adherent omentum, and possibly adherent intestine, and especially when attacks of marked intestinal obstruction have occurred, to perform the radical operation. In cases such as this it is surely better to run the small risk that radical cure entails to put the patient into a position of absolute safety rather than to wait until intestinal obstruction, with distended abdomen, vomiting, etc., occur, and then to do the operation for the relief of strangulated hernia under conditions so much more unpromising.

CASES 2 and 3. *Gummata of palate and of pharynx.*—I have here two female patients whom I want to show you together. They are both examples of tertiary syphilitic disease, affecting in one case the palate, and in the other case the

posterior wall of the pharynx. In the first place, as to patient number one. She sought advice at the hospital owing to the fact that foul-smelling masses of "stuff" were passing down the right nostril, and on account of very marked fœtor of the breath. She further reminds me that she had at the same time some lumps, two on the left forearm, and one on the forehead, which by their depressed and thin tissue-paper scars you recognise as being the cicatrices of gummata. We had, of course, to deal with undoubted tertiary syphilitic disease of the nose. She was treated with anti-syphilitic remedies and a nasal spray, but on her next visit she presented a perforating ulcer of the palate, a hole into which you could put your thumb. The patient's condition was very distressing when I saw her, and the state of affairs gave rise to very marked nasal speech, fluids passed into the nose, and the hole at the junction of her hard and soft palate was a source of very great trouble to her. She was given iodide of potassium in large doses for a long time, and the hole slowly contracted up. Still, there was always left a very large opening, so that some of her food almost always passed through her nose, and her voice was such that it was very difficult to understand her when she spoke. One similar case I saw was accustomed to plug the hole in the palate with cork, the opening being situated at the junction of the hard and the soft palate. That of course is one of the worst things which can be done, because constant pressure leads to enlargement of the orifice. In these cases it is of extreme importance not to begin plastic operation too early. In the very worst cases, where there is a very big opening, you can do nothing but advise the employment of an obturator made by a dental surgeon. The hole contracted slowly, and after cicatrization appeared to have narrowed the opening as far as it could do. I still waited, though the woman was impatient to have something done to close the orifice, which now admitted a lead pencil. Still, early attempts at plastic operations in such cases are apt to restart the gummatus process, and the whole wound may thus break down. After waiting until we thought it quite safe, I, a fortnight ago, made an attempt to close the orifice. It was situated at the most unfortunate place possible, namely, at the junction of the hard and the soft palate. As you will see if you look

into the mouth, I dissected a small muco-periosteal flap from the hard palate and turned it backwards, having previously freshened the edges of the opening. The exact method of operation was not a matter of very great importance, but some modification of the flap method was necessary, because one could not simply stitch up the round hole. Unfortunately a tiny piece of that flap has sloughed. However, the orifice is very much diminished in size (though the operation was only done thirteen days ago), and you will see that there is now only an opening which just admits a tiny probe. I believe cicatricial contraction will entirely close that. (This proved ten days later to be the case.) I think in about a fortnight the cure will be complete; if not, we shall apply the electric cautery to it. During the week following the operation the patient left off taking the iodide of potassium, and the tertiary manifestation of the disease began again in the nose. Her breath again developed the ozænatous odour, and she again noticed pieces of stuff coming down the right nostril. These symptoms have entirely ceased on the readministration of the drug. This case is interesting as showing the tendency for these cases of tertiary syphilis of the nose and palate to relapse. I have waited for nearly a year before attempting to close that orifice, and then you see that when I do make the attempt (probably owing to some irritation about the bone) the specific rhinitis starts again. I shall continue to give good doses of iodide, and then we shall hope that cicatricial contraction will close the orifice.

Patient number two is, fortunately for her, shown too late to be of much service to you. On Friday last, *i. e.* six days ago, this young woman had a very distinct gummatous ulcer on the posterior wall of the pharynx, and she has been on iodide so freely that it has now almost healed. I wanted to show you the case in conjunction with the former patient, and also because the situation of her lesion is not at all an uncommon one. A gumma starting in the periosteum of the pharynx, but more frequently in the submucous tissue, is not uncommon. The pharynx is a somewhat insensitive part right up in the naso-pharynx. Ulceration may proceed rapidly in these cases, and the septic organisms in the mouth cause the inflammation to spread with great rapidity, and destructive processes go on quickly. Unless rhinoscopic examination is employed one is very apt to think it

is merely a case of ulcerated sore throat, and to do nothing for a few days, and by that time perhaps irreparable damage may be done. This patient came a few weeks ago with a huge and painful ulcer, covered with dirty wash-leather slough and exceedingly inflamed. She came to the Aural Department three weeks ago, because she had earache, being unaware of the pharyngeal ulcer. The earache was pain in the ear due to septic inflammation in the pharynx. She was immediately put on very large doses of iodide, and fortunately the condition has healed entirely, except for a little depressed clean ulcer just behind where the uvula lies. The important point in these cases is their early diagnosis. As a rule they are only submucous gummata, and if they are treated with large doses of iodide of potassium at once, satisfactory results are almost always obtained. If, on the contrary, they are not recognised, but are left alone for some time, the ulceration is very apt to involve the palate as well as the pharynx. Cicatricial contraction occurs, and shuts off the palate and naso-pharynx from the mouth entirely; the soft palate being drawn back adheres to the posterior wall of the pharynx. That is one of the most intractable conditions you can have to treat, that is to say, total adhesion of the posterior margin of the palate to the pharynx. Even in cases where that does not occur you very often find cicatricial contraction produces a tremendous dragging upon the Eustachian tubes, and that is very apt to give rise to an incurable form of deafness. Now when I ask the patient to say "Ah!" she raises her soft palate, and you can see the depressed ulcer.

(To be continued.)

Epileptic Seizures and Uric Acid (Caro, 'Deutsche medicinische Wochenschrift,' May 10th). —A young man in good health, except for occasional epileptic seizures, was examined to determine the connection between the seizures and the elimination of uric acid. The curves show that the amount of uric acid in the urine fell progressively from .104 per cent. to .059 in seven days, and then abruptly rose again. Two severe seizures occurred the day it reached its lowest point. The amount of urine varied in an almost parallel curve during the sixteen days of the test.—*The Journal of the Amer. Med. Assoc.*, June 2nd, 1900.

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* Specially reported for *The Clinical Journal*. Revised by the Author.

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"CLINICAL MEMORIES,"

A LECTURE

Delivered at the Medical Graduates' College and Polyclinic, Chenies Street, W.C., Wednesday, 30th May, 1900.

By SIR W. T. GAIRDNER, K.C.B., LL.D., F.R.S.,

GENTLEMEN,—When the name was given to the subject of my address as it appears on the bill, namely "Clinical Memories," it struck me that some misinterpretation of it might occur; and this morning a friend said to me, "Oh, I suppose you are going to tell us some things about your old clinical teachers, Dr. Alison, and all the rest." No, that is not my object at all. No doubt a good deal might be said on that subject, but this is not the occasion to say it. The problem before my mind in reference to this address was how I should, being occupied up to the last moment at the Medical Council, speak to you in something of the spirit of your own institution here, and in a practical sense, making something like a contribution to your work. This is far from being an attempt at an eloquent lecture or anything ambitious; it is simply what is implied in a collection of clinical facts drawn from clinical teaching extending over a great many years, but subject to this condition, that I was obliged to select such points as could be brought within the compass of an hour, and at the same time such points as could be illustrated without the necessity of searching all over London for cases, which, of course, I could not possibly do, to bring before you.

I. PROGNOSIS OF MITRAL STENOSIS.

Without further preface, and without too much insisting upon detail, I shall first bring before you the subject of the prognosis of mitral stenosis—not the diagnosis; we have had plenty, perhaps, indeed, more than enough on the diagnosis, so I need not touch upon that. Upon this point of the prognosis, however, the experience of very

many years under circumstances where cases could be watched, sometimes over very considerable periods, has led me to believe that the gravity of the prognosis is greatly overstated in many of the books on the subject. That is rather an important matter, which should be cleared up. Now, necessarily from the nature of the inquiry that requires to be carried out over a long series of cases, and over a long time, I cannot insist upon the details, but my conviction after many years' watching is that the prognosis is not so grave as is generally believed. It has been alluded to on various occasions, and I think Sir William Broadbent will bear me out in saying that the usual way of presenting matters some years ago was that of all the valvular diseases of the heart mitral stenosis was the worst as regards prognosis. In maintaining that this is not so, I do not wish to be considered as desiring, by way of contrast, to understate the gravity of the prognosis; very far from it. All my preconceptions would have led me to believe that the prognosis was grave, and no doubt the prognosis is grave, and no doubt the mere fact of mitral stenosis leaves the patient in a very precarious state. But there are two or three things which aided my determination to look into that matter many years ago. The first was the publication of a book by Dr. Angus Macdonald upon 'The Bearings of Chronic Disease of the Heart upon Pregnancy, Parturition, and Childbed,' London, 1878.

Dr. Macdonald, who unfortunately perished of chest disease at far too early a period for his own reputation, had done very good work in Edinburgh as a gynaecologist. He wrote this volume to which I refer, and in it you will find it is written large all over the volume* that mitral stenosis is the gravest of all possible complications of pregnancy. Now it has happened to me now and then to see pregnant women whom I have either known before, or whom I came to know, suffering from mitral stenosis; and with Dr. Macdonald's book before me, had I trusted it implicitly, I should have been obliged to hold out in such cases an extremely alarming prognosis. But I found that many of

these women got on well, and some had two or three successive pregnancies, and did not appear to be much the worse. One I remember in particular was the sister of an old friend and pupil of mine, who knew all about the medical aspects of it as well as I did. We had a conference about her, and I told him my view of the much better prognosis that was admissible than that which Dr. Angus Macdonald's book would lead one to think. That lady went through several pregnancies very well, but she ultimately perished very suddenly by an accident—one of those which make for great uncertainty, not to say gravity, in the prognosis of this form of cardiac disease, namely, the formation of a thrombus behind the stenosed orifice. This led me to suggest to my then clinical assistant, Dr. Middleton, that he should keep this in view in the dispensary work of the Royal Infirmary, Glasgow, and he did so for a considerable time, and published the results in two very excellent papers in the 'Lancet,' October 26th and November 2nd, 1889, under the title "A Contribution to the Study of Chronic Valvular Disease of the Heart: being an Analysis of One Hundred and Fifty Consecutive Cases seen at the Glasgow Royal Infirmary Dispensary, with a special list of cases bearing on the influence of Pregnancy in Chronic Valvular Lesions," by Geo. S. Middleton, M.A., M.D., which I commend to your attention as being very satisfactory to my own mind, and yet as not having received, I think, the consideration that is due to them. In those papers you cannot fail to find that the very bad prognosis commonly assigned to mitral disease has got to be largely modified. Besides that I have had not only cases where repeated pregnancies have occurred since I knew of the existence of the disease in the patient myself, but I have had cases where, in women who had had a large family of children, it was exceedingly difficult to determine at what period of life the mitral stenosis had intervened. There were, perhaps, some little troubles mixed up in the history; but as there was in these cases no definite history of rheumatism or of any other known starting-point of a cardiac valvular lesion, it was difficult to get exact data. But in one or two of these cases I was almost led to arrive at the notion that the mitral stenosis might possibly be congenital. One of these cases I refer to was a very remarkable one, because it was inferred by me to

* See particularly p. 204, and the summing-up (1 and 2) in p. 207. I trust, however, it will not be supposed that I am in any way depreciating the practical value or interest of Dr. Macdonald's work, which, on the contrary, I hold in very high estimation.—W. T. G.

be one of mitral stenosis without there being, so far as observed, any definite murmur present. I saw the lady very late in her disease. She had been under the care of a medical friend some distance from Glasgow, and when she struggled up to Glasgow she was not exactly *in extremis*, but she was very urgently ill; and, as often happens in these cases, there was no murmur, but there was a wildly irregular action, and a small, almost imperceptible pulse. Altogether she was in a very bad way, and died in a few days. Being strongly impressed with this, and knowing that her previous history would be interesting, I had the curiosity to press for a post-mortem examination even after the body had been shut down in the coffin, and we obtained it. I found a mitral orifice that you could not even put your little finger into, but it was perfectly smooth—one of those funnel-shaped mitral contractions. The subsequent investigations showed me that she had been repeatedly for years under the care of her own doctor for cardiac troubles, but that he—I suppose from not hearing a murmur—had been inclined to “pooh-pooh” the whole thing, and led her to believe it was functional altogether. Therefore the net result to my mind was that you could not tell—you could not even guess—at what period the mitral stenosis had intervened in this case. But, as I said, she had borne numerous children,—quite a large family, in fact; moreover, she had had a great deal of distress, which was, according to the views entertained, a cause of the supposed functional disturbance. Her husband had become insane, and altogether she had pulled through as many domestic and other calamities as any woman need have to struggle through. Since then, and before that, I have had cases in hospital where the only thing you could depend upon even to suggest the occurrence of mitral stenosis at a certain date was the fact that hæmoptysis had occurred during sometimes several successive pregnancies in a way unknown previously. These pregnancies in other respects went on perfectly normally, and the patient recovered afterwards. That is only a small fragment of the kind of experience that I have had upon this subject, but I think you will agree with me in saying—though I do not wish to go too far—that it justifies a very considerable relaxation of the views that are commonly entertained about the specially grave prognosis of mitral stenosis.

II. THE AURICULAR-SYSTOLIC MURMUR (MITRAL) APART FROM ORGANIC CHANGE IN THE ORIFICE OR VALVULAR APPARATUS.

The next point I should like to allude to is what is commonly called Austin Flint's presystolic murmur. This is a point of diagnosis; but as I said before, I am not going to approach what we may call the ordinary questions connected with the diagnosis of mitral stenosis, because, to my mind, these are perfectly settled, and I have no more to say about them. An important observation was made by the late Dr. Austin Flint, published I think in 1865, that he had in two cases observed the peculiar murmur which I commonly call the A. S. murmur, commonly called in London, I think, the presystolic murmur of mitral obstruction, with an organically sound mitral orifice. He said he had seen two cases in which this occurred with a perfectly or almost perfectly normal mitral opening, but with a highly regurgitant state of the aortic valves. When I first encountered that statement I felt it was entirely opposed to my own preconceptions and, as I thought, to all my previous experience. It was almost inconceivable to me that such a state of matters should occur, and the explanation given by Dr. Austin Flint, though very ingenious, at first seemed to be more curious than probable. I therefore remained in a state of scepticism about it, and my scepticism was increased by the fact that Austin Flint himself had first observed these two cases at a period when, by his own statement, he had only recently become familiar with the presystolic murmur, *i. e.* shortly before 1865; and, according to the last edition of his book, before his death he did not seem personally to have met with any more. Since that edition, however, and since his death, individual cases have cropped up, so that I think we may say there are now perhaps a dozen or more. I think Dr. Sansom, in his admirable book on ‘Diseases of the Heart and Thoracic Aorta,’ gives as copious a reference to these cases as you will find anywhere else. It is on page 385 of Dr. Sansom's book. He has argued out the subject on the basis of Austin Flint's theory. I cannot afford the time to go into that theory in any detail, but the theory is this: that when the ventricles are filled from above the segments of the mitral valve are passively floated up in the act of so filling the ventricles

And he thinks that in a highly regurgitant state of the aortic valves, owing to the ventricle never being able to empty itself, that this process of floating up of the mitral segments occurs prematurely and is maintained until the auricle begins to contract, so that there really is an actual though not an organic mitral obstruction present at the beginning of the auricular contraction. A virtual obstruction occurs when the cusps of the mitral valve are thus floated up into the course of the blood-current, and it is sufficient to generate a murmur. Well, I must say the theory of the matter I am disposed on the present occasion to leave alone. It does not quite satisfy my mind; but if the facts are so we must either have some theory or say we have no theory. It is a remarkable fact that neither Dr. George Balfour nor I had ever seen a case which favoured Dr. Austin Flint's theory; and, as regards Dr. Balfour, I am led to believe that this statement still holds good; for in the last edition of his admirable book, published only last year, p. 120, he writes as follows: "Since Flint's day some dozen of similar cases have been placed upon record, none of them more convincing than his own. In regard to this matter I can only say that I never heard a presystolic murmur without finding after death an actual—not merely virtual—stenosis of the mitral opening; and also that free aortic regurgitation is so common an occurrence that if it could ever give rise to an auricular-systolic murmur these symptoms ought to be of greater frequency than they now are." That last remark always appeals to my mind very much. I have quoted from the last edition of Balfour's book, 1898, and I cannot look at this book without paying it the compliment not only that it is done very well, but it is done upon a different principle from some of your London books; it was not written in order to get practice, but rather it was done when he was on the point of retiring from active medical work. If very free aortic regurgitation could produce an auricular-systolic murmur, why is such a murmur in such an association not one of the commonest incidents, instead of one of the rarest? That they are among the rare incidents even of a large field of experience I think we may conclude, partly from Dr. Flint's own experience, partly from Dr. Balfour's negative statement, which I have read to you, and partly from my own. My own experience, as I have said, is that I have had

only two cases in which there was found anything like a resemblance to this murmur. Of course even one case which is absolutely clear in all its details would be sufficient. It so happens I have had two, which are not absolutely conclusive to my own mind; because they were both only a short time under observation, yet they certainly bore in that direction very strongly. The first one I published an account of in the 'International American Journal,' for 1888. The other case was that of Margaret G—, recently in the Western Infirmary, whose heart I now place before you. The brief abstract of the case, written in the journal during the life of the patient, and for purposes of teaching, is as follows:—"Age, forty-six. More or less palpitation for ten years, but more severe cardiac symptoms, ushered in by vomiting and purging, five weeks before admission. Dropsical swelling, pain, and breathlessness since that time. Blood passed *per anum* ten years ago, otherwise good health. No rheumatism. Eight children (youngest eight years old). One brother died of heart disease, and mother from chronic bronchitis. Alcoholic habit in family, and the patient herself admits it for ten years. Hypertrophy of left ventricle; rasping A. S. murmur, with blowing V. S. at apex. V. S. and V. D. murmurs at base. Sudden death seventeen days after admission."

Some details of physical examination.—"There is slight pulsation in the epigastric region. The apex-beat is felt in the sixth intercostal space, four and a half inches from the middle line. This is somewhat diffused, and a slight thrill is present which is presystolic in rhythm. On percussion the upper margin of the cardiac area is on a level with the upper border of the third rib. The right border is one inch to the right, and the left border four and a half inches to the left of the middle line. On auscultation, a rasping presystolic murmur is heard at the apex, followed by a blowing systolic murmur. In the aortic area and down the sternum a double murmur is heard, which is V.S. and V.D. in rhythm, the V.S. being loudest at the aortic cartilage and upwards."

Post-mortem report by Professor Muir.—"The heart somewhat enlarged, both sides being affected. The aortic orifice slightly incompetent (.9 inch): the pulmonic valve tight, mitral 1.45 inches, tricuspid 1.8 inches. There is a marked aortic endo-

carditis affecting only the two posterior cusps, and more especially at their junction. The left posterior has been partially separated, so that a fragment hangs down—hence incompetency. The right posterior cusp is also pendulous, but to a less extent than the left. At the beginning of the aorta there is a small aneurysm arising posteriorly above the two cusps mentioned, and projecting into the left auricle. The aneurysm without stretching is about the size of a small plum, and is almost divided into two by a septum corresponding with the line of junction of the two cusps. In the septum there is a small aperture. The lining of the aneurysm is smooth, apparently endothelium, and there is no thrombus present. The mitral valve is practically healthy; so are the valves on the right side. The orifices of the coronary arteries are apparently not interfered with. Length of ventricle three and three quarter inches, and the thickness half an inch to three quarters. Right ventricle also hypertrophied, and dilated. Heart muscle anæmic and friable, and there are some fatty changes. Weight of heart fifteen ounces.”

This case goes some length in confirming Dr. Flint's theory. I have the preparation here for those who care to look at it, and I will state shortly what it is. It is a case in which there were small aneurysms above the aortic valves with incompetence of the valves; the aneurysms protruded towards the mitral side, and it is just conceivable that the aneurysms themselves might have a somewhat constrictive effect upon the mitral orifice.

III. ADDISON'S DISEASE AND LEUCODERMA, OR VITILIGO.

The next subject I want to say something about to-day is the connection of leucoderma with Addison's disease. Possibly some of you may, and others may not, recollect a very early instance of this association, which is of some historical interest. It is No. XI plate in Dr. Addison's famous monograph on 'Disease of the Supra-renal Capsules,' 4to, 1855. I show you here the book and the plate. There is no doubt it was done from a rough sketch, but it is an evident case of leucoderma, which Addison himself at least must have included in the book because he thought it was a case of disease of the supra-renal capsules. But unfortunately he had not a post-mortem in this

particular case; and therefore he puts it side by side with another case in which there was indeed a post-mortem, but where he had not managed to secure a drawing. He makes the two cases do duty jointly for the presentation of the facts and the inferences as regards one of them. It is curious that Dr. Wilks, writing in Reynold's 'System of Medicine,' puts his finger upon this case and says because it was of a leucodermic order it is not a true case of Addison's disease. In short, he disputes Addison's own view of it. It is Case 6. Now a good many years ago I had a case sent to me very late in the disease, when the patient was extremely ill, for an opinion. I said at once "That is Addison's disease, it can be nothing else I think." There was no other kind of organic disease to be discovered at this time; yet the patient was obviously dying with all the usual symptoms of Addison's disease. It was, then, to my mind a case of Addison's disease, but it was also a glaring case of leucoderma, or, as it is sometimes called, vitiligo, with pigmentation largely of the usual Addisonian type, but interspersed with circular, and sometimes coalescent, patches of vitiligo. That opinion I maintained until the patient's death. A post-mortem examination showed the usual form of disease of the supra-renal capsules. Dr. McCall Anderson, who had seen the case at an earlier period, had spotted it as a case of leucoderma; but I do not know whether at that time he had made up his mind that there was any sufficient evidence of Addison's disease. However, the case is published in the last edition of his book on 'Skin Disease;' so you can look it up.

Not long after this the International Medical Association met in London, in 1881, and Dr. Headland Greenhow read a most elaborate paper upon Addison's disease; and you all know how careful Dr. Greenhow was, not only as an observer, but as an almost exhaustive commentator on the literature of a subject. In reference to Addison's disease he may be said to have left almost nothing unnoticed up to the date of his research, which is now published in a volume accessible to most of you. This paper was an abstract of the whole research, admirably done (see 'Transactions,' 1881, vol. ii, p. 59). At the end of the paper I raised a question and stated briefly the leading facts of the case just referred to, which had not at that time been published. I brought forward no theory, but

thought the opportunity was a favourable one for ascertaining if anybody else at home or abroad had anything to say on the matter. We got no response. I alluded to the fact of Dr. Wilks having differed from Dr. Addison himself about the significance of leucoderma, as shown in Dr. Addison's own book, and suggested that it was a point requiring to be more carefully dealt with. But there was no information forthcoming, and therefore we are bound to assume that no one there present (and it was a large assemblage) had anything to show. I have not seen many cases, but I have seen two or three more or less suggestive of the same thing, and I happen to have at this particular time one such case under my care.

The patient is a woman who is tuberculous; she has tuberculosis of the lungs beyond all doubt. I can go no further now than to briefly state that she herself recognised a darkening of the skin as simultaneous with the development of the symptoms of bodily debility before there was any distinct phthisical element in the case. On the back of that darkening of the skin came a whitening in particular patches of the skin. I have made a most careful description of the whole thing in my ward journal, and I have the advantage of being able to show you in the first place a coloured drawing of the case, which was done by a friend, and here are also two photographs which reached me since I came here, one showing the front and the other the back of the body of this patient. There is no difficulty whatever in identifying the disease as leucoderma, with general and apparently Addisonian general pigmentation. The only question is, "Is it a case of disease of the supra-renal capsules?" That, I need not say, cannot be very positively settled during her lifetime, and at present she is rather improving than otherwise, even as regards the phthisis. Dr. Radcliffe Crocker has kindly lent me a drawing of a case of vitiligo, which I have not seen myself yet, and this also I shall hand round. I met Dr. Crocker in an informal way yesterday and told him about this, and he said he had been of opinion for some time that in leucoderma darkening of the skin precedes the lightening of it usually; that the darkening comes first in order, and then the working out of leucodermic patches follows the darkening. That he holds to have been the case in the portrait which is seen here, but which has no connection with Addison's

disease. That would be a very interesting point for further observation and discussion.

IV. SMALLPOX. CORYMBOSE AND OTHER VARIETIES OF ERUPTION. THE "ECTROTIC" TREATMENT.

I have not time to speak of much else, but I want, in the time that remains, to say a few words about smallpox, of which, fortunately, one does not see much in recent years, but in the early years of my hospital practice, and even after going to Glasgow in 1862, I happened to see a great deal of it. I do not know how many of you here have seen much of smallpox. But if you study the subject you must have been struck, as I was, with the description of one particular kind of eruption by Dr. Marson, which you will find in the article upon the subject contributed to Reynolds's 'System of Medicine.' I allude to what Dr. Marson there calls the corymbose variety of the eruption. What he says about the corymbose variety is that it is a very singular and a very fatal form of the disease; and that it is rather rare. I confess that in my much smaller experience it has been so rare that I am even doubtful of having ever seen it. Dr. Marson, however, from his vast experience of thirty years has collected 104 cases, twenty-nine in unvaccinated persons, seventy-four in the vaccinated, and one after inoculation. The mortality was about equal in the vaccinated and unvaccinated, 44 per cent. in the former, 41 per cent. in the latter. This very high rate of mortality, moreover, is, in Dr. Marson's own words, "out of all proportion to the amount of pustulation which rules so powerfully in other forms of the disease; why it is so it is impossible with our present knowledge to say, and it is probably one of those things that will for ever remain inexplicable." In other words, there seems to be no reason (according to Dr. Marson) from the character or the abundance of the eruption why it should be so fatal; it is not hæmorrhagic, it is not confluent; in short, it is not anything which should mark it as of fatal type, but he says that as a matter of fact the mortality is far beyond all other varieties of not very profuse eruptions. I cannot say I have seen any case of corymbose smallpox corresponding to Marson's description, but I had a very curious opportunity once of seeing smallpox coming out in a boy who had previously had psoriasis, and the

distribution of the pustules is very clearly shown in the drawing now before you. I think you will at once recognise its analogy with what Dr. Marson would call the corymbose form; the eruption is not confluent, not even very profuse on the whole, but it is crowded on in patches where the psoriasis had already established an unnatural state of the skin in patches beforehand. Well, that was an isolated case, and I did not attempt for a time to generalise from it. But when I was a very young clinical observer there was an epidemic of smallpox in Edinburgh, and just about that time in France there was a theory set up that if you could keep the atmospheric air away from smallpox pustules you would abort them. My then teacher, Professor John Hughes Bennett, took up this theory very warmly, and the treatment that was based upon it. This was called at the time ectrotic treatment. Following up the views of the French physicians, Dr. Bennett caused to be made all sorts of masks to cover the faces of smallpox cases, with the view of aborting or sending back the eruption on the face. Shortly afterwards, when I had occasion to treat some cases of smallpox in advance of the eruptive period, and at that time collodion had just come into use, I took the notion of painting over experimentally certain limited patches of the surface with collodion, so as to observe for myself how the eruption would turn out. I chose cases where, from the epidemic conditions and associations of the patients, I was sure they were going to turn out smallpox, but during the pre-eruptive stage. I thus hoped to ascertain definitely if the ectrotic treatment had any great influence upon the appearance or abundance of the eruption in those parts of the body where it might be presumed that, if left to itself, it would have come out symmetrically, and with a certain regularity. After I had gained confidence by working on the arms and legs with collodion in this way, I carried it a stage further and painted one half of a man's face with collodion, and left the other half unpainted, and I confess I was rather in a funk lest he should get one half of his face full of smallpox and the other half with none, and that he might bring an action for damages against me for deforming his face. But as a matter of fact, when he emerged from the disease the two sides of the face were almost exactly alike. Therefore I made up my mind that the ectrotic treatment was based upon false patho-

logy altogether, or at least that it had no foundation in fact. But in view of this case of psoriasis, and in view of what has not infrequently been observed as regards the coming out of the eruption of smallpox with special profusion on blistered surfaces of the integument, another probable view of treatment has occurred to me. To carry it out on a large scale I have suggested to my classes, but only as a theoretical opinion, that one might be able, instead of driving back—or aborting—the pustulation (as supposed to be done in the so-called ectrotic treatment) to attract the eruption out. You cannot drive the eruption back, and probably if you could you would injure your patient. But you might probably, I think, attract it away from some parts of the body where it is specially inconvenient, *e.g.* the face; that is, supposing there is in each case a fixed quantity of smallpox which must needs come out. This may be theoretical, and I have no proof to offer you, but the idea may be worth entertaining, and I have always said that if I happened to see cases in the midst of a smallpox epidemic of persons who were almost sure to have taken smallpox, but in whom the eruption had not yet come out, I should be inclined to put on mustard plasters or blisters upon the epigastrium, or otherwise to stimulate and even irritate the surface anywhere away from the face, or anywhere they suffered pain in the hope that the eruption would come out in sufficient abundance there and be diminished at other places. I have here three drawings in connection with smallpox which I thought might be worth presenting. Here is one which at the time of its occurrence I presented to the Medico-Chirurgical Society of Edinburgh, and I am bound to say my opinion was received with a good deal of scepticism; but I still think it is a case of smallpox eruption upon the colon. I need not go into the history of it, but it was a fatal case which was accompanied by a good many dysenteric symptoms. But the look of the mucous membrane satisfied my mind that it was a case of smallpox eruption on the mucous membrane of the colon. The other two drawings are accurate representations, admirably coloured, of a state of the kidneys which was very unfamiliar to me, being at that time Pathologist to the Edinburgh Royal Infirmary; nor can I say definitely that I have seen anything exactly like it since. I very much doubt whether anybody here has seen

anything exactly like it, but I shall hand the drawings round for your consideration. The kidney is in an acute process of degeneration with complete necrosis in parts, that is to say, gangrene, but without any offensive odour. The case was that of William S—, a patient in the Edinburgh Royal Infirmary, or rather in the Cholera Hospital, in 1848. It was a case of fatal cholera which closely—*i. e.* within a few weeks—succeeded smallpox. The lesion was, I think, due more to the smallpox than to the cholera. It must be a very rare case, as I have already said, but as it falls naturally under the head of my discourse, and as the drawings are in themselves worth showing, I simply hand them round without further remark.

V. PERICARDITIS.

I had intended saying a few words about pericarditis, but I think that subject may easily lead me too far. Here is a very good example of what is sometimes called hæmorrhagic pericarditis. When aneurysms have burst into the pericardium, which they sometimes do, the patient surviving the immediate rupture, I have occasionally seen the contents so churned up as to present something like the appearance in the drawing. The question has arisen in my mind, May some of the cases of hæmorrhagic pericarditis be something other than pericarditis? Is this form of disease really a pericarditis at all? or is it not rather, in some cases, a bleeding into the pericardium, the blood having deposited its fibrin owing to having been so churned up?

Here is a rather interesting illustration of pericarditis. The drawing I now show is of two distinct cases. The right hand one is of a fringe of very chronic lymph on the surface of the ventricle, projecting almost like the ends of a brush, I thought at the time evidently an adhesion torn by the movements of the heart, and leaving evidently a permanent roughness. This case has no clinical history, but the other drawing on the left hand is, I think, a very curious and interesting one. It represents a case which was under my own care, one of somewhat chronic pericarditis with effusion. The lymph formed after death had probably been originally rough, shaggy, or of the usual tripe-like character; but the conditions had lasted long enough to cause all the most exposed surfaces of this lymph to become almost smooth—polished, in fact, by attrition, like a pebble. You see that

the greater part of the lymph on the anterior surface has been smoothed down by multitudinous friction carried on probably for several weeks or months. The patient was admitted late in the disease, and although we were practically certain that there was pericarditis with diminishing effusion, and the case was very closely watched from day to day, we were only able to hear a friction-sound once, and then by no means very definitely. The fluid effusion at death was by no means large, but we had the conviction that at an earlier period there had been a much greater amount of fluid, and probably (though we cannot know) at a still earlier period a loud friction-sound, such as is suggested by the great amount of fibrin. But during our cognizance of the case (as already said) friction was rather indistinctly heard, and only heard once.

VI. RUPTURE OF ANEURYSMS.

Now let me say just a word upon perforations of aneurysm upon mucous surfaces. I have gone into that subject at some length in the article on Aneurysms, first in my 'Clinical Medicine,' and then in Dr. Clifford Allbutt's 'System of Medicine,' so I need not dwell very much upon it; but I will just briefly show you, because I may not have another opportunity before a Society like this, a drawing which is engraved both in my 'Clinical Medicine' and in Dr. Allbutt's 'System,' showing what I call the pinhole perforation of aneurysms on a mucous surface, sometimes two or three such perforations close beside each other, and consistent with life being maintained for some time, the bleeding going on in small quantities into the air passages. That life may be maintained for some time under these circumstances is still more emphatically shown by another drawing, which has also been engraved for the articles which I have referred to. It is much more striking in the coloured representation. This was a case which I had very early in my practice. It is a case of lobular condensation of the lung with muco-sanguineous infarction from pressure of aneurysm. It is an aneurysm opening into the left bronchus, which pumped its blood into the bronchus, the blood being then aspirated into the lung, and so producing an almost chronic and, as you see, a lobular infarction. That this process had been going on for some time was shown by the changes which had taken place in the lobular condensation, which

at some places was bright red or purple, as is common in pulmonary hæmorrhages connected with cardiac disease, but at other points devoid of red colour, being yellow or yellowish grey, hard, and indurate. I apprehend that the proof of the time element here is apparent in the drawing itself.

VII. CEREBRO-SPINAL MENINGITIS.

This case occurred to me at a time when I was Pathologist to the Edinburgh Royal Infirmary, and when I was not acquainted clinically with cerebro-spinal meningitis; nor had I any direct knowledge of the case during life. The point which struck me most about it on making careful inquiries after the post-mortem investigation was that nothing like a diagnosis had been made during life. I am not blaming anybody; it was a long time ago, and there is nobody living to be blamed; but as a matter of fact the poor girl who was the subject of this disease went through the hands of five or six leading medical men in the clinical wards of the Royal Infirmary, and was treated for all sorts of things, but not for cerebro-spinal meningitis. Among other things she got into the gynæcological ward, and as she had an undeveloped womb she was treated for that, or for nervous troubles, perhaps hysterical, supposed to arise from retarded sexual evolution, though I cannot say what were the symptoms which led to that discovery. The only man who came near a diagnosis was my dear old teacher, Dr. Alison, who said, I think, that there would be found some "effusion of the brain." It is only an illustration of what I am afraid is a common enough thing, medical errors.

VIII. DERMANYSSUS AVIUM (?).

The only other thing I propose to detain you with, as I do not like to exceed my time, is this communication, which I hunted up in my letter-book of years ago, when thinking over this address. I think it explains itself pretty well. In July, 1879, I got this letter from a very experienced old practitioner in a leading town in Scotland, whom I have known since his student days, and whom I knew to have one of the largest practices in that part of the country:—"My dear Dr. Gairdner,—Can you tell me of any consultant who could give special advice in a distressing case of a lady who in visiting the poor has become infected with lice. I

never met with a genuine case of phthiriasis before. I have had her bathed over and over again, each time putting on clean clothes. Once the bath in this way was twenty minutes soaping and scrubbing, then twenty minutes sponging all over with Solutio Sulph. ē Calce, then a third twenty minutes in the bath. Also lotion of carbolic acid to limited portions. All her clothes have been disinfected at a temperature of above 300°. Still the loathsome creatures reappear. The brutes are very small. I enclose one (don't be alarmed, he is in gum so cannot get out)."

Thus far my friend's letter, which gives a very vivid picture, I think, of the consternation produced by this unwonted phenomenon in a person presumably of cleanly habits. Well, having thus got hold of a *corpus delicti* (as the Scotch lawyers say) I submitted these tiny creatures to several experts. We easily determined they were not pediculi of any of the three varieties proper to man. They were not lice in the ordinary sense of the word, but we were at first considerably puzzled to know what they were, and it was only after a good deal of searching that we found there was a creature called *Dermanyssus avium* which presented a certain resemblance to the specimens.* I had drawings made of them at the time, but they have gone astray, or at all events are not to be found with the letter just quoted. But I made a shot that it was the *Dermanyssus avium*. I therefore wrote to my friend and suggested that the supposed pedicular infection might be found to have been got from poultry. It turned out that this poor lady was a great fancier of poultry, like a certain princess, and, like that princess, found them give her a great deal of trouble. But, unlike the princess, she was in the habit of making herself extremely familiar with the fowls and nursing them in her lap, hence the propagation of this peculiar disease. I wrote to the gentleman in question to know if he had ever heard of any more examples. I find he has not, and, therefore, I do not think it can be a very common thing.

* Dr. Finlayson, who kindly interested himself in this case at the time, and saw the specimens and drawings made by our late friend Dr. John Wilson, sent me the following reference:—"In 'Descriptive Catalogue of the New Sydenham Society's Atlas of Skin Diseases,' by Jon. Hutchinson, Part 2, London, 1875, p. 87, *Dermanyssus avium* or *D. gallinæ* is described and figured from specimens obtained from a woman."—W. T. G.

WITH
MR. HERBERT WATERHOUSE
IN THE WARDS
OF CHARING CROSS HOSPITAL.

(Concluded from p. 144.)

CASE 4. *Carcinoma of breast simulating gummatous ulcer.*—The next patient I want to show you has lost to a considerable extent the characteristic appearance, but it is a case of a woman about which I for some considerable time have been in doubt as to whether we had to deal with gumma of the breast or a carcinoma. Oddly enough, the microscopic examination has rather been more confusing than helpful. Her age is fifty-five, and for some months past she has suffered from an incurable patch of eczema on the left breast. She then developed a round painless swelling about the size of a walnut in apparently the subcutaneous tissue of the breast. After a time the skin ulcerated, and when I first saw her there was a frightful stench and a large wash-leather looking slough at the base of the ulcer. It was almost a perfectly round ulcer. She had been treated elsewhere as a case of gumma of the breast, and I can candidly say I never saw anything more like gumma of the breast. A fortnight's treatment with iodide did not result in any real improvement. Gummata of the breast usually resolve rapidly under large doses of iodide of potassium, but this practically remained *in statu quo*, or perhaps was a little worse. One or two enlarged and tender glands could be distinctly felt in the axilla, but that taught us nothing, because in a septic ulcer of the breast you expect of course to find glandular enlargement. Still in doubt, but inclining towards the diagnosis of carcinoma, I placed the patient on the operating table, with permission to do what was necessary. When she was under the anæsthetic I put in a sharp spoon and scraped out a layer of necrosed tissue more than half an inch in thickness. It appeared a typical, dirty, foul, wash-leathery gummatous slough. I then felt much more hopeful than I had done before, that we had to deal with a case of gumma, particularly as the microscopic report was that the

tissue was simply a mass of necrosed tissue. She was put back to bed to be treated with iodide again, and for about a week the condition seemed to be improving. The cavity certainly got smaller, and we began to hope that we had an example of very chronic indurated gumma of the breast. Now, however, unfortunately, in spite of the fact that the cavity has been kept perfectly clean, and in spite of the administration of large doses of iodide, the induration has spread, and so markedly that there is now no attempt at healing. I am afraid now that there is no shadow of doubt that we have been misled, and that the condition we have to deal with is one of cancer of the breast, not gumma, as we hoped it might be. As I have said, microscopical examination of the slough which I removed has misled us. The report was that it was simply necrotic sloughing tissue, without any sign of cellular structure whatever. I should like you to examine and feel the breast. I am afraid we have lost valuable time, but I fail to see what other course we could have adopted.

[Later note: The breast was removed by my senior colleague, Mr. J. H. Morgan, and proved to be a carcinoma.]

CASE 5. *Intense neuralgia of the inferior dental nerve treated by neurectomy.*—This man presents a most interesting case, in which the result so far of operative treatment has been of a very satisfactory nature. This patient for two and a half years had been the victim of most agonising neuralgia over the area supplied by the right inferior dental nerve. The pain has been so bad that it has prevented him from working most of the time, and he tells us it has sometimes almost driven him to commit suicide. These intense neuralgias of the fifth nerve produce some of the most terrible sufferings to which the human frame is liable. He has had practically every drug that could be used in the treatment of neuralgia. He has been into the Dental Hospital for relief, and it was from there he was sent to me by Mr. Percy Smith for neurectomy, as nothing in the way of extracting teeth had had any good result. The neuralgia was limited to the distribution of the inferior dental nerve, that is to say, it involved the teeth, the gum of one half of the jaw, the corresponding half of the chin, and the lower lip. There was intense hyperæsthesia all over this area. I performed neurectomy by trephining the ramus of the lower jaw with a half inch

trephine. The great thing in this operation is to avoid, as far as possible, injuring the branches of the facial nerve. In making incisions one avoids the nerves more than any other structures, and this is especially true of the face. My incision started three quarters of an inch below the lobule of the ear, coursed around the angle of the jaw, and continued forward along the lower margin of the body to the anterior margin of the masseter muscle. Having cut down right on to the bone, I, with knife and periosteum detacher, divided the insertion of the masseter muscle, and Mr. Clogg forcibly retracted upwards all the soft parts down to the bone, including the facial nerve and its branches. We had to stretch the facial nerve tremendously, but we did not cut it. I trephined with a small half-inch trephine, placing the pin of the instrument exactly over the central point of the ramus of the jaw, and came upon the inferior dental nerve just as it was entering the inferior dental foramen. No damage, of course, was done to the margins of the bone; I only made a hole in the centre of the ascending ramus. The inferior dental nerve was at once exposed, and an inch of it was excised. The soft parts were then replaced, the fibrous insertion of the masseter being sewn to the periosteum of the lower margin of the jaw. Next day the patient had almost total paralysis of the lower part of the face. He could shut his eye, but had paralysis; he could not whistle. Still, I was not unhappy about it; I felt certain we had avoided cutting the facial nerve, and I knew it was the forcible retraction which caused the temporary paralysis by dragging on the branches of the facial nerve. In a very few days the facial paralysis began to diminish, and in ten days he was able to whistle. Now he will demonstrate to you that he can whistle, and there is no trace of facial paralysis. His condition, he will tell you, is a very pleasant one after all his suffering. I hope the relief from pain may long continue. In many cases neurectomy proves an unsatisfactory operation, the pain coming back again at the end of a few weeks or months. But where the pain has been accurately limited to one nerve, and you can take a good inch out of that nerve, my experience is that in at the least two cases out of three there is freedom from recurrence of neuralgia. You must therefore make certain that you have got your section well on the proximal side of the nerve required to be cut. If a man has pain in the inferior dental and

auriculo-temporal, it is no good dividing one nerve and leaving the other. One further point about the case is that I stitched his masseter down again; healing by first intention took place, and there is absolutely no interference with the movements of the jaw. You can see that he is able to open his mouth wide enough to admit three fingers placed one above the other, and to close the mouth firmly and rapidly.

CASE 6. *Charcot's joint disease.*—The next case I want to show you is one of extreme Charcot's tabetic arthropathy, and it is doubly interesting because it exhibits in the same patient two forms of the joint disease. Here is a patient with locomotor ataxy of definite syphilitic origin. In my experience all cases of locomotor ataxy are syphilitic in origin; certainly 95 per cent. of the cases are. I need not demonstrate the case fully; you will see that the patient has loss of knee-jerks, spinal myosis, Argyll Robertson pupil, and, he tells us, lightning pains. Therefore we have here all the essential points which enable us to make the diagnosis of tabes with absolute certainty. I want to direct your attention particularly to his right shoulder and his left elbow.

The history is that, as usual in these cases of tabetic arthropathy, we have first an effusion—a fairly rapid effusion—into the joint, but without any pain and without any rise of temperature. Then, after the effusion, we have the bones taking on, as a rule, one of two actions. Here is a shoulder in which we have markedly atrophic changes. You will feel that there is practically no head to the humerus, for where the head ought to be the bone is no bigger, but rather smaller in diameter, than the shaft. There is in this patient almost a condition of dislocation, and there is such atrophy of the muscles that in such a case one might get all kinds of dislocations. In the shoulder there has been simply atrophy of the head of the bone and of the muscles and ligaments. Here at the elbow (and that is the feature which makes the case so interesting) there are enormous osteophitic growths. Here the process, instead of being atrophic, is hypertrophic, for there is marked enlargement of the bones entering into the formation of the joint, and in addition the presence of large irregular bosses of bone. These osteophitic changes in tabes, however great they are, rarely lead to ankylosis. It is remarkable that, in spite

of his condition, this patient is able to go about and do his work (he is in the employ of the vestry, and uses the broom as a road-sweeper a good deal). One can see on his thumbs and fingers the large callosities which are caused by the pressure of the broom, probably owing to the abnormal way in which he has been obliged to hold the broom. He has had lightning pains, but the condition of the affected joints is not markedly worse than when I last saw him a year or two ago.

CASE 7. *Gangrenous partial enterocele (Richter's hernia); resection of intestine, with use of Murphy's button; recovery.*—This case is one of the most interesting, certainly one of the most fortunate cases I have ever had. The patient was brought into hospital one Friday afternoon late, with a tremendous brawny induration on the right side of the abdominal wall, extending upwards nearly to the level of the umbilicus, and downwards well below the groin into the thigh. The induration was evidently an acute inflammatory condition, obviously of septic origin, and the subcutaneous tissues were enormously swollen. The history he gave was that he had had a rupture on this (right) side for quite two years, and then suddenly he had been seized with pain about the region of the umbilicus, which was very severe, and completely doubled him up. That was nine days before he was admitted. For the first six or seven days he had more or less diarrhoea. It stopped on the Wednesday, and he came in on the Friday following. He had been passing flatus all the time. He had vomited several times. Strangulated intestinal hernia was a condition which one hardly thought of, because the man told us he had had several motions, certainly the first six or seven days out of the nine, and on the day he was admitted he had passed wind. His abdomen was absolutely flat. No lump could be felt in the groin owing to the intense inflammatory infiltration of the abdominal wall. I must say that the diagnosis which occurred to me as the most probable was that he had no bowel strangulation, but that there was a gangrenous omental hernia. I felt that the patient's condition must proceed from some gangrenous inflammation, owing to the intense diffuse cellulitis of the abdominal wall. But I did not diagnose the true condition, namely, that it was a case of Richter's hernia or partial enterocele, where part only of the circum-

ference of the bowel is nipped in the hernial orifice, but not the whole lumen of the bowel is so occluded. In this case there must have been only the slightest channel, that is to say, a channel allowing only fluid fæces and flatus to pass. The patient had a temperature of 104° , and he was about as bad as he could be. We rapidly took him into the operating theatre, and cut through this intensely oedematous skin and subcutaneous tissue (I should think we went three inches through the oedematous skin and subcutaneous tissue). We then came upon a certain amount of fæcal-smelling pus,—not very much, perhaps two ounces—and then we encountered a gangrenous femoral hernia. It was only comparatively small, that is to say, about one inch in length, and I thought at first that it was the vermiform appendix, as it looked just like it. It was black, and when I touched it it came away in my fingers; it was absolutely gangrenous. At last I recognised the condition with which I had to deal. The *Bacillus coli communis*, with perhaps other microbes, had passed through the gangrenous bowel wall and inflamed hernial sac, and had got into the subcutaneous tissues of the abdominal wall. This infection had caused the intense diffuse cellulitis. The question now arose, what were we to do? I thought the case was about as desperate as it could be, and I thought the horrible gangrenous cellulitis of the abdominal wall would cause death by blood-poisoning. I did not wish to divide the stricture only, because I felt that the bowel with the large hole in it would slip back into the peritoneal cavity; in fact, just as we did divide the stricture the bowel did nearly slip back into the abdomen. One good point about the case was that the intestine was not distended, and therefore we could stitch it. If there is one golden rule in surgery, it is never resect distended intestine. Owing to the fact that the obstruction had not been total here there was no distension. Having divided the stricture I drew down the intestine, found that the hole in it was more than one and a half inches in length, and nearly an inch in breadth. In fact, there was almost no bowel in the gap, except a tiny margin along the mesenteric attachment. The man's condition did not admit of much time being wasted, therefore, as the intestine was not distended, I cut out a segment of bowel about three inches in length, including the hole and an inch of

congested œdematous bowel on either side of the opening, and united the intestine by means of Murphy's button. But we were not over our difficulties yet, because when I tried to put back united intestine containing the button I found the hernial orifice was not big enough to give passage to it. I had to divide the orifice upwards, and in doing this I divided an abnormally placed obturator artery, which had to be ligated. The bowel was placed just inside the internal opening of the crural canal. Now I had to attempt to shut off the peritoneal cavity from infection from this virulent cellulitis. I stripped the peritoneum surrounding the crural ring with the finger, detaching it from the abdominal wall, and pushed it in a sort of funnel fashion into the abdomen. I turned thus the peritoneum inwards into the abdominal wall, leaving a gap between peritoneum and abdominal wall, which we packed with double cyanide gauze. We hoped that if we could keep them together for twenty-four hours we might get peritoneal adhesions shutting off the general peritoneal cavity. The peritoneal surfaces were not, of course, face to face. Fortunately this acted perfectly, and we had no peritonitis. The only thing which was at all amiss was that two or three days afterwards part of the infiltrated cellulitis suppurated, and the house surgeon, Mr. Clogg, had to open the subcutaneous abscess and stuff it. Now the man is absolutely well, and has been back at his work. He passed a Murphy button on the ninth day. In the cases I have had in which I have used the Murphy button it has always been passed between the eighth and twelfth days. There are probably few more fatal affections in surgery than gangrenous Richter's hernia. Owing to the absence of total obstruction hernia is not thought of, and consequently these cases are very apt to go on until gangrene takes place.

CASE 8. Multiple lipomata of subcutaneous tissue.—This man is the subject of multiple lipomata. You will notice that he has five tumours; he has one of them behind each ear, where they are loose in their capsules, and they give a marked feeling of fluctuation. If you pick one up you will notice that the skin puckers, owing to the fact that the trabeculae of the lipoma is in union with the fibrous tissue of the skin. He has two much larger ones lower down at the back of the neck, and a very big one under the chin. These are

scarcely amenable to surgical operation. In the lower ones you cannot note distinctly where the lipoma ends and the ordinary subcutaneous tissue begins. These diffuse lipomata are certainly best left alone, though they are slowly increasing in size. He has had them for at least five years. Of course when one cannot remove tumours of this sort one looks about for any drug which will be of benefit. We have tried in this case liquor potassæ in 20 minim doses, three times a day. This certainly seems to have checked their growth, though I do not think we can say they are much smaller. I have found in several cases that while the patient was taking liquor potassæ these tumours apparently did not grow. I have known patients who have taken it for months and in whom the growths have not increased but have slightly diminished in size. The back of the neck is an extremely common situation for them. In these lipomata I repeat you cannot tell where they end and the normal subcutaneous tissue begins.

CASE 9. Lipoma and para-umbilical hernia of sudden formation.—This man, aged seventy-seven, presents a case of some little interest, because it exemplifies the very slow growth of lipomata. There is a lipoma over the manubrium sterni of twenty years' standing, and it is growing only very slowly indeed: he does not think it has grown any larger in the last four or five years. You will notice very clearly the dimpling of the skin over the growth of which I spoke to you in the other case. Even when I make the skin quite tense you will notice a certain amount of dimpling. An interesting point about this patient is that he presents an example of very suddenly developed umbilical hernia, which came down only on Friday last. This patient has been troubled for some time with a winter cough, and on Friday he gave a sudden cough and forced down this umbilical hernia. The hernia is reducible, and quite resonant on percussion. The point I want to impress upon you is that which I have already mentioned once, namely, that umbilical hernia of adults is hardly ever at the umbilicus. In the infant the umbilicus is the weakest part of the abdominal wall, and therefore it stretches and the rupture comes through. But once let the umbilicus cicatrise down, it becomes the strongest part of the abdominal wall. Therefore when umbilical hernia does develop in adults, it

is in the middle line, either above or just below the umbilical orifice. This is really not an umbilical hernia, but a ventral hernia about three inches above the umbilicus. It is interesting as a case of sudden development of ventral hernia. It is also interesting on account of the fact that here is a lipoma which has existed for twenty years and apparently almost ceased to increase in size.

CASE 10. *Traumatic gumma*.—This patient is another instance of the condition which I have described to you before under the name of traumatic gumma. I do not understand why it is, but I think I can safely say that, though I meet with a case of traumatic gumma on an average, I should say, quite once a month, yet the fact that traumatism frequently plays an important part in the localising of a gumma is scarcely now generally admitted, or even understood. I am sure many of you have seen cases which I have shown on other occasions in which we have seen distinct traumatic gummata. I do not say that traumatism does more than localise the gumma—*i. e.* if a man is going to have a gumma, and has an injury, the gumma is more likely to form at the place injured than it is to form elsewhere. That is only what one would *a priori* expect. This patient has a swelling, which you will see below and in front of the angle of the scapula, and over the greatest convexity of his ninth, tenth, and eleventh ribs. It is firm and hard, and softer near its central part. I want you to notice that it is but little painful now, because since he has been taking iodide of potassium the pain has diminished considerably, and there is less redness. He had syphilis four years ago. Four months ago he had a blow there—a fairly hard knock, he says,—and very soon afterwards he noticed a lump. As a matter of fact, frequently in these cases, before the hæmatoma has had time to disappear, the gummatous infiltration comes on. This is in the subcutaneous tissue; it is in a part where the skin is thick and where there is but little irritation. This has lasted for four months without much change. I am certain we shall now find that under considerable doses of iodide of potassium this gumma of the subcutaneous tissue will disappear entirely. After only four days of treatment by iodide of potassium the growth is less painful and smaller, besides being less inflamed. We have also to note the fact that he came with three

gummatous ulcers on the scalp and forehead, and these, owing probably to the pressure of his hat, had broken down. I saw him for the first time four days ago, when they were red and inflamed, and very sore. In four days they have begun to heal, and the pain which attended them has practically entirely disappeared. The gummata on the scalp are of the ordinary kind, but this one on the body is a very firm fibrous gumma, in which we should probably find that the gummatous infiltration had organised in considerable part into fibrous tissue. I hope to show you this case again next time, by which date I think the growth will have entirely disappeared.

Note.—Three weeks later the lump had almost entirely disappeared, and the gummatous ulcers of the scalp and forehead had healed under iodide of potassium.

CASE 11.—*Tertiary syphilitic sarcocoele*.—I think this next case is interesting because of the way in which the diagnosis can be made without any doubt. I want to impress upon you, in cases where the diagnosis of syphilitic disease is doubtful, *i. e.* in chronic diseases of the nervous system, the necessity of looking at the testis as one of the most likely spots to see the tertiary syphilitic disease. I think I may safely say that certainly, as far as I, a surgeon, am concerned, the commonest situation for finding a gumma, after the skin, subcutaneous tissue, and bone, is the testis. Over and over again in the diagnosis of obscure cases of cerebral syphilis (that is to say, cases in which one has been doubtful whether the patient has ever had syphilis), I have received great help from an examination of the testes. This man's history is that he is twenty-six years of age, and that he noticed in March of this year that his left testis was beginning to increase in size. There was no pain in it, and there never has been any, the only discomfort was that it felt heavier and larger, and seemed to be in his way when walking. The swelling of the testis developed within a month, unaccompanied by pain, though there was some dragging on the cord. Another most important point was that there was loss of testicular sensation. The testis was found enlarged generally, the epididymis was evidently flattened out behind the testis, and one could not tell where the testis ended and the epididymis began. The two forms of tertiary syphilitic disease of the testis which often

go together are :—(1) gummatous infiltration, that is to say, a diffuse gummatous infiltration of the tunica albuginea and trabeculæ of the testis, of which we had an example here, where the testes felt uniformly greatly enlarged; (2) localised gummata, where there are one or more distinct sharply demarcated gummata. Where there are localised gummata, therefore, there are apt to be topical swellings on the testis. Remember, as I have said, the two processes may go together.

The diagnosis in this case was never in any doubt, for with those points which I have referred to before one's mind a mistake could not be made. The patient was accordingly put upon large doses of iodide of potassium, and the swelling of the testis then became markedly reduced, and in addition testicular sensation was rapidly regained. We now notice that the testis which was affected is scarcely any bigger than the other, the epididymis is felt quite distinctly, especially below, and the testis is normally sensitive. The only thing to observe now is that the epididymis at the upper part of the testis is still indurated, but the body of the testis and the globus minor are normal.

The two conditions which one is apt to confuse with tertiary disease of the testis are : encephaloid cancer, and tubercle. Tubercle, of course, chiefly affects the epididymis, it is of slow growth, and there are often signs of tubercle elsewhere. The body of the testis usually escapes in tubercular disease, the epididymis being alone infected. Sooner or later the skin becomes adherent, we have also caseous nodules forming, and the cord is sooner or later bound to be involved. Testicular sensation is lost only very late in the disease. I should say that in tubercular disease the testis very rarely reaches anything like three or four times the normal size. In encephaloid cancer, of course, the growth is extremely rapid, and instead of loss of testicular sensation there is acute pain, the skin becomes infiltrated, and death from secondary deposits occurs almost always within a comparatively few months. There is never any improvement of this condition under treatment. The improvement which this man has obtained from iodide of potassium leaves no doubt about the nature of his case.

CASE 12. *Sarcoma of scapula, treated by partial resection of scapula.* This man's condition is interesting from the point of view that he shows a

very satisfactory result of operation for sarcoma of the scapula. He is sixty-one, and works in Woolwich Arsenal. For the last four months he has noticed a swelling occupying the upper angle and part of the supra- and infra-spinous fossæ of the left scapula. Apparently it was not a very malignant form of sarcoma. It was hard and it had not grown very rapidly; the skin was not infiltrated, and we thought we could move the trapezius over it. It had not any very marked adhesion. There was no doubt that the man's only chance lay in removal of the growth. Now removal of the whole scapula is a formidable operation, and chiefly dangerous on account of hæmorrhage, and when the scapula has been removed, the arm is not of very much use. Of course you take away the glenoid, and the shoulder-joint disappears. Owing to the fact that it did not seem to be a very malignant growth, I thought we would make an attempt to save the use of the arm by retaining the glenoid, the acromion, and the coracoid. I accordingly, by the large crucial incision which you see, removed the body of the scapula, leaving the bony parts above mentioned. This partial removal of the scapula was attended by, I think, more hæmorrhage than total removal; but, thanks to good assistance, this was readily controlled.

At this late hour I must not trouble you with the details of the operation. The result is, as you see, that the man has an exceedingly useful arm, instead of a very useless, very wobbly limb. One little accident occurred. Nowadays one reckons on keeping every wound aseptic. Here, where you have an enormously big wound, where it is necessary to have numerous assistants, where there are two or three nurses handling sponges and instruments, where you want the artery forceps quickly, almost quicker than they can be handed to you—there is some risk of infection. This patient was on the table less than three quarters of an hour. However, some septic organisms did gain entrance; I think almost certainly from a saw which I called for hurriedly, and which I have since learned was not thoroughly sterilised. Still this really had no serious untoward result. The man's temperature rose to 105° at the end of the fourth night, and therefore Mr. Clogg, the house surgeon, cut two or three stitches, and, introducing dilating forceps down to the sawn section of bone, evacuated two or three

drachms of pus with excellent result. The sepsis had been so slight that it did not really interfere with the healing of the wound, except just at the space where the drainage-tube had emerged. The greater part of the wound healed by first intention, and the result is all that one could desire. You will see that this patient can perform almost any movements, except that he cannot abduct the humerus above a right angle. He can, however, do almost everything else, and his limb is a powerful, useful one. He is leaving the hospital to-day, and is returning to work as a stoker at the Woolwich Arsenal. The result has surpassed my expectations. I show you the growth. It is being prepared as a museum specimen.

Ambulatory Treatment of Fractures.

C. D. Lockwood, in the 'Illinois Medical Journal' for April, 1900, says the ambulatory treatment of fractures was recommended by Berard as early as 1833. Among modern surgeons Hessing was the first to successfully employ this form of treatment. Surgeons have been slow to adopt the method. The writer defines the ambulatory treatment to be the application of such devices as will enable the patient to be put in an erect posture while bony union is taking place. He thinks that this method is especially applicable in fractures of the leg in children and those of the femur in adult persons, and in all simple and uncomplicated fractures of the lower extremity in young adults. The theoretical dangers from the ambulatory treatment of fractures are pressure sores and muscular contractions. An examination of the published cases shows that embolism is no more likely to occur when the patient is about than when he is in the horizontal position. In fact, those fractures accompanied by extensive injuries to the soft parts and comminution of the bone are exactly those in which the ambulatory treatment is not to be advised. Pressure sores are due in every case to ill-fitting appliances, and if these occur the surgeon is responsible. Contractures can be overcome by careful attention to the after-treatment and the application of massage as recommended by foreign surgeons. The dangers attending ambulatory treatment are largely theoretical, and may be practically disregarded. The early application of ambulatory apparatus, with massage is the ideal treatment for fracture.—*Medicine*, June, 1900.

A CASE OF ERYTHROPSIA.

BY

MAYNARD GABE, L.S.A.Lond.

AN after-effect of witnessing the solar eclipse was as follows:—A. B.'s attention was drawn to the partial eclipse, which was then visible. He, however, was unable to see anything as the sun was too strong, and he did not possess any coloured glass at the time, but he waited for an intervening cloud which came in the course of a few minutes, and enabled him to make it out with the naked eye. The cloud passed quickly, and the sun appearing again in its strength left A. B.'s vision rather confused. He reached home, however, and by that time his sight had become normal. He again looked at the eclipse through coloured glasses, viz. dark red, blue, and smoke-coloured, but the one mostly used was the blue one. The eclipse over, there was absolutely nothing to attract A. B.'s attention to any defect in his vision until it became dusk four or five hours later. At dusk a signal-box which was then being lighted appeared red, so much so that A. B. remarked to a friend that it was illuminated for some reason or other. His friend remarked that he noticed nothing unusual. However, it then transpired that all artificial lights appeared red, and the farther the distance the more marked did the colour become. On nearing the object, such as an ordinary street lamp, this colour disappeared, and the light appeared natural. This state of affairs existed for two days, but only in the evenings. He was not the subject of cataract, and possessed normal vision. He regained his normal vision on the third evening, and has been quite free since. This case, however, differs from a true case of erythropsia, inasmuch that it was only the lights in the evening appeared red, whereas in a true case all things, I believe, appear so. No treatment was undergone.

WE have received from Burroughs Wellcome & Co. specimen samples of Urotropine in tabloid form. Urotropine, which is also known as formin, is particularly useful in genito-urinary therapeutics. The amount of urine is increased there is a corresponding advance in the quantity of uric acid excreted, and further it can be shown that the solution of the urates is capable of demonstration in the course of a few hours after the taking of Urotropine. Medical men will do well to remember this particular tabloid, as it certainly is well spoken of by those who have tried it, and that is more than can be said of many of the newer remedies.

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TWO CLINICAL LECTURES ON LEUCOCYTHÆMIA.

Delivered at the London Hospital,

By PERCY KIDD, M.A., M.D., F.R.C.P.,

Physician to the Hospital and to the Hospital for Consumption and Diseases of the Chest, Brompton.

LECTURE I.

GENTLEMEN,—I have selected the subject of leucocythæmia for a clinical lecture for one or two reasons. In the first place, it is not a very common disease, and I therefore ask you to take careful notes of the cases as you come across them. During the last two years it happens that I have had several interesting examples, and I have thought them worthy of being brought before you. They are not only good cases, because they illustrate the ordinary form of the disease, but they illustrate some rather unusual points to which possibly some importance may be ascribed. What I propose to do is to describe briefly the nature of the disease, and then before going into the cases to say a few words about the blood in general; because, without having some intelligent understanding of the normal cells of the blood, it is impossible to appreciate the present position of the pathology of leucocythæmia.

Now we may define leucocythæmia as a progressive form of anæmia characterised by changes in the blood, both qualitative and quantitative, and associated with lesions of the spleen, bone marrow, and lymphatic glands and other organs. To criticise that definition is not difficult, for anæmia, as we know, is merely a symptom. We are still obliged to define certain diseases as anæmias, though we know that this is not the whole truth; we do not know what the cause of the anæmia is. I am afraid we must say that in the case of leucocythæmia we are as yet ignorant of the essential pathology of the disease. It is very possible that the changes in the blood which

I shall describe directly are merely the result of some occult form of poison.

Leucocythæmia is a disease of comparatively recent history. It was discovered in the year 1845 almost simultaneously by Hughes Bennett and Virchow. Professor Bennett described the condition as suppuration of the blood, because the blood contained an enormous number of cells like pus-corpuscles. Virchow, perhaps, took a more accurate view as regards the pathology of the disease, inasmuch as he described the disease as leucæmia, or white blood, and attributed the changes in the blood to an increase in the number of the colourless corpuscles or leucocytes. For some time Virchow's classification of the disease was generally adopted. He divided it into two groups, the splenic and the lymphatic. In the splenic form, according to Virchow, the leucocytes of the blood were mostly of large size; whereas in the lymphatic form the cells were smaller, and in this form of the disease the lymphatic glands were generally, if not always, much enlarged. In the year 1870, Neumann, from a study of the development of the blood, came to the conclusion that the starting-point of leucocythæmia in many cases, if not indeed in all, was not the spleen and not the lymphatic glands, but the bone marrow. After a great deal of discussion it was generally recognised that there were three forms of the disease—the splenic, the lymphatic, and the medullary. For some time no further progress was made in our knowledge of the disease. But comparatively recently Ehrlich, who has done so much for chemical microscopy, instituted a classification of leucocytes according to their chemical or staining properties, and on this basis proceeded to study the relations of the different leucocytes to certain diseases. In one class the granules in the protoplasm stain with "acid dyes," such as eosine, in another with "basic dyes," like methylene blue. The former are called "oxyphile," the latter "basophile" cells. A dye is said to be acid or basic according as the staining depends on the acid or basic element of the aniline salt. In normal blood, as you know we find several different kinds of leucocytes. The most common form is the so-called multinuclear, in which the nucleus is much split up and looks rather like a tangled skein of worsted, as Dr. Rose Bradford very aptly described it.

The multinuclear appearance is not due to the presence of a number of nuclei, but to the irregular formation or subdivision of the nucleus. It is believed that this multipartite condition of the nucleus is the result of extremely active movements of the protoplasm surrounding the nucleus. In other words, these polynuclear cells, as we may call them for the sake of convenience, are very highly amoeboid. In normal blood over 70 per cent. of all the leucocytes consist of polynuclear cells. Originally Ehrlich described these cells as neutrophile, *i. e.* cells which contained granules staining with a mixture of an acid and basic aniline dye. But Kanthack and others showed that the essential part of the dye which stains is the acid part, and hence they are truly oxyphile cells. But before going further I must remind you of what I daresay many of you know, that when one speaks of the cell being oxyphile one means not the nucleus, but the protoplasm of the cell. This is a most important difference, because you will notice in any of these preparations that the nucleus stains with methylene blue. In the polynuclear leucocytes the nucleus stains with methylene blue, but the surrounding protoplasm acquires a pink tinge with eosin. With a powerful microscope you will find the latter colour is due to the presence of minute pinkish granules.

The next most common leucocyte is the lymphocyte, or small hyaline cell. Lymphocytes are like the cells found in lymphatic glands, small cells with a round nucleus which occupies a large part of the protoplasm. In many specimens from dried blood they seem to consist of nothing but nucleus, you can make out no definite ring of protoplasm around them. In the fresh state, and in many film preparations of blood you can make out a fine ring of protoplasm round the nucleus, and the protoplasm is called hyaline because it is wanting in chromatin, and does not stain appreciably with any dye. These lymphocytes or small hyaline leucocytes are present in normal blood in proportions varying from 10 to 20 per cent.

There is another form of large hyaline leucocyte, to which the name myelocyte has been given, but it is doubtful if this is ever found in healthy blood. The myelocyte is a large cell which has a single nucleus, and in that respect

it agrees with the lymphocyte. But the nucleus occupies a relatively smaller part of the cell, and is commonly more or less reniform or indented, and often lies close to one pole of the cell. But in preparations of leucocythæmic blood you may find that the nucleus is not typically reniform, and occupies the centre of the cell. These cells are commonly hyaline, that is to say they stain very little with any dye at all. But in some cases of leucocythæmia the protoplasm may stain finely with eosin or methylene blue, and at times both pink and blue granules may be recognised in the same cell.

The next and only other important leucocyte found in the blood is called the coarsely granular eosinophile cell, a cell somewhat larger than the polynuclear variety, whose protoplasm is studded with a number of coarse red granules in eosine preparations. These large oxyphile granules also stain with osmic acid, but they are not fatty, for they are not dissolved by ether or alcohol. The nucleus is single, and varies a good deal in position and size. The average number of the eosinophile cells does not exceed 2 per cent. in normal blood.

These are the most important hæmic leucocytes. There is another uncommon form, described as basophile, which shows a fine granulation with methylene blue staining.

We may now pass on to leucocythæmic blood. First of all let me say a few words about the different leucocytes. We find in most cases a large number of the polynuclear cells and myelocytes. The latter, as a rule, show a marked relative and absolute increase, in contradistinction to the polynuclear leucocytes, which are relatively diminished. The number of lymphocytes varies considerably. In one form of leucocythæmia, of which I shall show you an example, the increase of leucocytes is almost confined to this one particular form of cell.

Lastly, we find a varying number of eosinophile cells. These differ as a rule from the typical eosinophile leucocyte of healthy blood, the cell being larger and the nucleus more like that found in the myelocytes, which they closely resemble.

I now show you on the screen a few photographs taken from blood-films. The first is from a very typical case of leucocythæmia in which the spleen was greatly enlarged. It shows a large

number of myelocytes and many quite similar cells with coarse eosinophile granules. The eosinophile cells look extremely black on the screen. You will see that the polynuclear cells are present in considerable numbers. In this specimen there are a considerable number of lymphocytes.

The next is a very remarkable specimen. It is a case in which the increase of leucocytes is almost entirely due to the lymphocytes. You see there are also some large myelocytes, but the greater number of the leucocytes are lymphocytes; in fact, the lymphocytes amounted to rather over 90 per cent. of all the leucocytes present. The case presents this interesting feature, that there were absolutely no polynuclear cells in any part of the specimen, a fact that I do not remember to have seen recorded elsewhere. The next slide is a very interesting one. In this specimen the polynuclear leucocytes amount to over 80 per cent. of the total number, which is much more than usual, a relative as well as an absolute increase.

In leucocythæmia, as a rule, the red corpuscles undergo a progressive diminution in number. In certain cases, especially where anæmia is most marked, they exhibit changes such as we find in other severe forms of anæmia, that is to say irregularity of shape (poikilocytosis), diminution of size of individual corpuscles (microcytes), and a varying number of nucleated red corpuscles. The hæmoglobin is always more or less diminished, and generally in proportion to the reduction in the number of red corpuscles.

After this brief and imperfect introduction, I will give you a short account of one or two cases to illustrate what I have been speaking about.

The first case illustrates well the ordinary form of leucocythæmia, to which the name spleno-medullary is given. But before I speak particularly about the case I must explain what is meant by these terms. You will remember that originally great stress was laid on the enlargement of the spleen, and Virchow thought that it was owing to some perverted function of the spleen that the increase of leucocytes took place. Then you will remember the lymphatic glands were considered to take part in the production of the disease in its lymphatic form. And lastly Neumann came forward and maintained that the

marrow of the bones was primarily at fault. When we speak of the splenic form we mean that the spleen is greatly enlarged and shows marked changes. The term "spleno-medullary" expresses the general opinion that although the spleen may show the most pronounced changes, the bone marrow is the tissue in which the essential changes originate. In the same way when we speak of lymphocythæmia or lymphatic leucocythæmia we do not mean that the lymphatic glands necessarily are the starting-point of the disease, but that in this particular form there is an increase in the lymphocytes of the blood.

CASE 1.—*Spleno-medullary Leucocythæmia.*

Sarah M—, a young woman twenty years of age, unmarried, was admitted on September 4th, 1896, and the following is the history which was obtained:—Family history unimportant. The patient had never had any illness before the present with the exception of chronic constipation. There had been no injury or epistaxis. The present illness began ten weeks before admission with gradual swelling of the abdomen, and "a tumour was felt on the left side" as she said herself. This tumour gradually increased in size, and at times she had some pain over it. The patient also stated she had become pale and had lost flesh. On admission she was pale, but not intensely anæmic. The dulness of the heart was increased upwards to the left, and a systolic murmur was heard over the pulmonary region. Examination of the blood showed a great increase of the leucocytes, 100,000 leucocytes and 3,000,000 red corpuscles in the cubic millimetre. With regard to the other organs, the liver was moderately enlarged, the spleen was greatly enlarged, and it reached downwards to the right as far over as the right iliac fossa, and on the left side extended into the pelvis. The splenic dulness reached up into the axilla as high as the seventh rib. The patient stated that the catamenia developed at the age of fifteen, and were perfectly regular until ten weeks ago, from which time they had ceased altogether. Ophthalmoscopic examination showed nothing beyond pallor. The temperature of the patient was always above the normal varying between 100° and 103° . Her blood was frequently examined, and certain slight variations occurred. Three weeks after admission

the red corpuscles, which had been 3,000,000, fell to 2,470,000 and the white corpuscles rose to 273,000. The hæmoglobin was 36 per cent. The patient was then put on arsenic in increasing doses, and improved slightly. Four months after admission the report reads, red corpuscles, 3,230,000; white corpuscles, 220,000; hæmoglobin, 25 per cent. The course of the illness was uneventful. On a few occasions the patient had epistaxis. As I have said, her temperature remained persistently elevated, and as a rule it was remittent, never reaching the base line. For a week or two the temperature was intermittent; the morning temperature being normal or slightly subnormal. The patient was ultimately discharged at her own request, and was lost sight of six months after admission. From a number of blood-films the leucocytes were estimated as follows:—polynuclear cells, 47 per cent.; myelocytes, 36 per cent.; lymphocytes, 13 per cent.; cells with coarse eosinophile granules, 4 per cent.; nucleated red corpuscles in fair numbers. Many of the myelocytes contained fine oxyphile granules.

CASE 2.—*Acute Lymphatic Leucocythæmia; No enlarged Glands; Moderate Enlargement of Spleen; Purpura; Epistaxis; Bleeding from Gums.*

Mabel S—, aged five, was admitted July 11th, 1894. *Family history.*—Father has sallow skin and is liable to bleeding from nose, mouth, and rectum. Is said to have enlargement of liver and spleen. Paternal grandmother was subject to bleeding. The history was that the child had measles three years previously, but had made a perfect recovery from the disease. For some time before admission to the hospital the child was noticed to be ailing; it got easily tired and "lay about," and did not play with the other children. She attended a Children's Hospital for general weakness, but was not laid up until a week before admission, when a number of red spots came out on her legs and spread over the body. At the same time the abdomen was noticed to be swollen. A day or two before admission she bled from the nose. On admission she was somewhat anæmic, and the skin was covered with a general purpuric eruption. There was a slow but constant oozing of blood from the gums and from

the nose. The abdomen was full. The liver could not be felt. The spleen was moderately enlarged. The blood was examined, but not very completely, owing to the stress of a very large "take in." A very great increase of leucocytes was found, but the exact number was not estimated. Fortunately a number of films were taken, and were examined after the patient's death. We found that 90 per cent. of the total leucocytes consisted of lymphocytes, the other 10 per cent. being myelocytes. No polynuclear leucocytes could be found in any of the specimens. No nucleated red corpuscles.

The lymphocytes in this case differed slightly from the ordinary form; they were not so sharply stained as usual, but they were rather larger than the average, and the nucleus stained in a paler way with methylene blue. The progress of the case may be briefly summarised as follows:—Slow bleeding from the nose and gums continued, blood was vomited and passed by the bowel, and death took place from exhaustion on July 15th, four days after admission. The temperature varied from 99° to 101°, never reaching the normal.

Post-mortem examination.—Hæmorrhages in the visceral pleura, pericardium, lungs, and stomach. Heart contracted and healthy. Spleen, 7 oz., large and firm. Malpighian bodies easily seen, and surrounded by a thin dark reddish zone. Kidneys, 5 oz., very pale, some hæmorrhages under the capsule. Sternum and ribs showed no naked-eye changes. No enlargement of lymphatic glands.

I pass round a set of sections in which you will be able to see with an ordinary lens a few interesting points. The medullary cavities in the sternum are filled with deeply stained tissue, and with the microscope you will see that this consists of accumulations of lymph-corpuscles like those in the blood. Similarly stained areas in sections of the liver and kidney correspond to leucocytic infiltration of the portal canal and cortex respectively.

So far we have described two cases of the spleno-medullary form, in which the spleen is certainly enlarged and where, according to the prevailing views, there is a change in the medulla of the bone. But we had no post-mortem in the first case. In the second case which I de-

scribed to you there were changes in the blood, but almost exclusively concerning the lymphocytes, without any polynuclear cells, and without any very great increase of the large hyaline cells of myelocytes. I shall have something further to say about the classification of this disease. We shall find that the course of the disease varies very much in the two forms. We can divide them for practical purposes into an acute and into a chronic form.

CASE 3.—Spleno-medullary Leucocythæmia; Excess of Polynuclear Leucocytes; double Calculous Pyelitis; Priapism; Death from Cellulitis of Arm.

F. S.—, aged 30, a sawyer, admitted on July 6th, 1895. The history he gave was that he was born in Whitechapel and had lived all his life in Stratford. He had had no illness until two and a half years before admission, and at that time was an in-patient in this hospital for nephritis. On referring to the notes we found that the patient had acute nephritis, with œdema of the legs, scrotum, and back. The liver and spleen were normal. There was no history of ague, gout, rheumatism, or syphilis. The present illness began six weeks before admission, with pain in the left hypochondrium and swelling in the same region. He became languid, short of breath, and unable to walk. On admission he was intensely anæmic and of a curiously sallow tint, but his nutrition was fairly good. There was no tenderness over the bones. The lymphatic glands in the axilla were slightly enlarged. The liver could just be felt below the ribs. Spleen very large and firm, lower edge one inch and a half below the umbilicus. Heart's apex-beat in the fourth space, no murmurs. Blood, red corpuscles, 2,750,000; leucocytes, 250,000; hæmoglobin, 45 per cent. Lungs free. Nervous system normal. Ophthalmoscopic examination gave a negative result. Urine, acid, specific gravity 1010, albumen $\frac{1}{10}$, much pus, hyaline and epithelial casts. After admission the patient complained so much of pain in the left side, and the pus in the urine was so constant, that I asked Mr. Moullin to see the case with me, and he agreed that it was justifiable to explore the region of the left kidney with the hope of finding something which could be relieved. Mr. Moullin

went for his holiday very shortly afterwards, and in his absence Mr. Dean undertook the operation. He exposed the left kidney, and passed his hand freely all round it. He found no enlargement of the kidney, the whole of the swelling being due to the spleen, which was adherent to the neighbouring parts. The wound was stitched up, and no marked hæmorrhage occurred. Two or three days after the operation blood began to ooze from the wound, but the bleeding was easily checked. The progress of the case was very instructive. The patient continued to complain very much of pain in the left side of the abdomen, and the urine very frequently contained uric acid crystals. Albumen was always present, but pus was not always to be found. He also had a curious symptom, which has been described in connection with this disease, a most painful and distressing priapism. It has been thought that this is due to thrombosis taking place in the sinuses of the corpora cavernosa. I do not know whether that is so, but the priapism passed off in two or three weeks. In November, 1895, an abscess formed in one of the patient's fingers which he had accidentally wounded. Following on this he got extensive suppuration in the hand, which slowly spread to the forearm and reached the axilla. Mr. Hutchinson saw the patient. The arm was freely opened in various places, and treated by boracic baths and other measures, but never with any complete relief. The patient slowly became more feeble, and in December, that is to say a month after the first suppuration, an ischio-rectal abscess formed, which was opened, but very little relief ensued, and he slowly sank just before Christmas, 1895. In October, the patient having been on arsenic almost continuously, the number of red corpuscles rose to 3,800,000, but the leucocytes had also increased—450,000. Examination of blood-films showed polynuclear leucocytes, 83 per cent.; myelocytes, 10 per cent.; lymphocytes, 7 per cent.; no eosinophile cells; nucleated red corpuscles numerous. Towards the end the patient had a purpuric eruption on his legs. It is somewhat remarkable that the temperature was never raised until the last few weeks, and even then it never exceeded 100°. On post-mortem examination by Dr Schorstein a very interesting state of things was found. There were enlarged glands in the

axillæ and groins. In the right lung was an old patch of tuberculosis at the apex. Heart, 12½ oz., recent vegetations on the mitral valve without any thickening. Spleen, 5 lbs., soft, surface puckered, numerous yellow infarcts. Kidneys, 12 oz., capsule very adherent, in the pelvis of each kidney a large uric acid stone. The kidney substance was very fibroid, and one kidney contained a large number of cysts. The calculi, which were of considerable size, did not completely obstruct the ureter. The mesenteric glands were enlarged. The condition of the blood is very interesting. There was both a relative and absolute increase of the polynuclear leucocytes. This may possibly be connected with the long-continued suppurative pyelitis, for in the leucocytosis met with in cases of suppuration it is the polynuclear cells that are increased.

In my next lecture I shall refer to the pathology of the disease, and consider some other very interesting cases which we have had in the last two years occurring in children which were closely allied to leucocythæmia, but about which there is still some doubt.

A WEDNESDAY CONSULTATION AT ST. GEORGE'S HOSPITAL,

JUNE 13th, 1900.

MR. BENNETT.—This is a very interesting case. The patient has been in the theatre for consultation before, after having been examined carefully by myself and others. She had been complaining for some months of abdominal pain, which was thought by the practitioner who sent her to the hospital to be due to some form of chronic intestinal obstruction, a conclusion which was amply justified by the symptoms at that time. The most careful investigations failed to reveal any abnormal abdominal condition. It was decided, therefore, as we could not find any evidence of abdominal disease, that the symptoms of which she complained might have been due to some neurotic or other functional condition, and that the best plan of treatment was an expectant one—at all events for some little time, to see if any

thing further would develop. Shortly after the first consultation, on again examining the patient, as had been done before, there could be felt in the right side of the belly a very distinct tumour, which can be readily felt now. It is either a gall-bladder full of stones, or it is a kidney. It is somewhat difficult to say with certainty which of these conditions is present; but that there is a tumour in the abdomen admits of no doubt whatever. The swelling can be made to go back into the loin, and suggests a kidney. Yet the shape is not that of a kidney, and we know that the gall-bladder does sometimes extend back into the loin and may resemble a kidney to the touch. It is, of course, important to have an idea as to which of the two things mentioned it may be, with a view to deciding upon the best way in which to approach the tumour. As there is some doubt as to the nature of the tumour, I think the proper course is to explore it by an incision through the anterior abdominal wall in order to make sure of its nature, and then to deal with it according to circumstances. If it could certainly be said that the tumour is not the gall-bladder, I should approach it from the loin. My own feeling, however, is in favour of making an anterior incision. The patient is quite determined upon having an operation; there is therefore no difficulty in that respect. In favour of the tumour being renal, there is a point which I forgot to mention, namely, some pus has been found in the urine. Clinically the case is of great interest, inasmuch as it shows how impossible it is to give anything like a sound opinion in some of these abdominal cases without repeated thorough examination.

MR. HAWARD.—I agree that it would be right to cut down upon this tumour and see what the nature of it is, and I should advise incision from the front. The symptoms are more like those of renal tumour than anything else. But after all, if there be a renal tumour it can be dealt with from the front, and as there is some doubt the anterior incision would, in my opinion, be the best.

MR. BENNETT.—Here is another interesting case in a woman who has also been in the theatre for consultation before. The case was originally admitted under the impression that the patient was the subject of gastric or duodenal ulcer. She had all the symptoms of that condition; from time to

time there had been vomiting, and she stated that she had vomited blood; on several occasions she had passed stools which were very black. At the same time there had been no evidence of the vomiting of blood beyond the patient's own statement. I brought her up for consultation because she appeared to be in a bad condition generally, and it seemed to me to be a case in which it would be justifiable to make an abdominal exploration, to ascertain the real condition of affairs. She was fairly nourished, but was suffering from persistent pain and a little tenderness around a point midway between the ensiform appendix and the umbilicus, but a little to the right side, just at the spot, in fact, at which the tenderness and pain in these gastric ulcers and other conditions of the sort occur. After consulting upon the case it was decided pretty unanimously that there was not sufficient evidence to justify exploration; the case was therefore set aside for treatment by dieting and other temporising methods. In order that she might remain as long as possible under observation she was sent to the Convalescent Hospital at Wimbledon, being at that time a fairly nourished woman, a little neurotic perhaps, and suffering from symptoms of gastric or duodenal trouble, but not in a critical state in any sense. She returns from Wimbledon in the condition you now see her—emaciated, weak, and looking very much more ill than before. Her abdomen, which was a little full, has become retracted; the tenderness still persists. Up to the time of her going away she was under careful dieting and rectal feeding, and so long as she was fed in this way no vomiting occurred. But a return to feeding by the mouth was followed directly by vomiting, and now she continues to vomit whenever she takes anything in any way, excepting by the rectum. There is no dilatation of the stomach, but there is intense irritation of it from some obscure cause. I can feel no tumour, like a pyloric mass or induration, or anything of that kind, nor can I find anything towards the cardiac region which would lead one to suspect the existence of carcinoma or anything of the sort. But we have all seen this woman before, and have consulted about her, and we not long ago decided that there was not sufficient objective evidence to justify any surgical proceeding. Three or four weeks which have elapsed since then have brought

about the change which you now see; she presents symptoms of some grave internal lesion, although still there is nothing objective to be felt. Her age is twenty-nine. The same question arises now as arose at the first consultation: is this a case in which an abdominal exploration should be made or not? It is quite clear that the woman is very ill, and that her trouble now cannot depend merely upon some neurotic condition. It seems to me therefore that unless we can do something for her she is not likely to recover. She is, it is true, not in a good condition for operation at present, but she is better than she was when she first came back from Wimbledon, owing to the increased nourishment which has been given her by the rectum. My own feeling is that an exploration is the proper proceeding, but before doing this it would be well to try to get her into a better condition than she now is by further rectal feeding. If, however, she makes no progress I propose to make an exploration at once, although I am fully prepared to find nothing obviously wrong inside the abdomen. Moreover, if nothing abnormal is found, I believe, if she survives during the forty-eight hours following immediately upon the operation, that the treatment will relieve her of her symptoms and practically effect a cure.

MR. HAWARD.—The symptoms seem to be almost entirely referable to the stomach, and I think it would be quite right to make an exploratory examination of the kind you suggest. Of course, people who are of a neurotic temperament do get emaciated quickly sometimes, but as a rule I have thought that any severe change of this sort has, at any rate, an organic basis, however much the condition may be exaggerated by the nervous condition of the patient. I should advise operation.

MR. BENNETT.—Here is another interesting case from the point of view of treatment. It is that of a middle-aged man who slipped when going down some steps and broke his patella. That happened on June 8th. About that fact there is nothing remarkable. He has a transverse fracture of the patella, with but little separation, the lateral expansions about the joint not having been torn to any extent. At first sight it would naturally be said that the case is an admirable one in which to apply the ordinary treatment by wiring. Indeed, so far as the local conditions are con-

cerned a better case could hardly be chosen. But the point which leads me to bring the case up for consultation is that the patient is an epileptic whose fits are frequent and violent. The fits occur about once in ten days or a fortnight. He happened to have one of these convulsions the day after he came into the hospital, and so violent was it that he broke the splint which was placed on the back of the injured limb, and there was very great difficulty in controlling him. Yet notwithstanding the violence the separation between the fragments is very small. The question for consideration is this: which is the better plan, under the circumstances, to treat the case by wiring, or to treat it without an open operation, seeing that the limb is liable to such violent action during the fits? It is quite a difficult question to decide. Seeing that the lateral expansions have not been torn, in spite of the violence caused by the fit since the injury, my feeling is in favour of treating this case without operation. At the same time I do not see any great objection to making an operation, because I think in a case like this the wire should hold, provided that free passive movement be employed from the time of operating. If I operate I propose to wait until the occurrence of the next fit, and then to proceed with the operation as soon as is possible afterwards, so that at least ten days would probably intervene before another convulsion. He is under a regular course of Bromide three times a day, but the fits seem little, if at all, affected by the drug.

MR. HAWARD.—A man who is liable to epileptic fits would have a more secure patella if it were wired than if it were treated in the ordinary way with a more elastic or extensible material between the ends of the bone. I think his future comfort and the utility of his limb would be better ensured by wiring the patella than by the other methods of treatment; but of course the epileptic fits import a complication into the case. Still, as Mr. Bennett has pointed out, the lateral expansions of the patella are intact, or nearly so, and I should not think there would be any material risk in treating him by wiring.

MR. DENT.—I should be inclined to wire it; and I do not see why the case should not do very well.

MR. DENT.—I show you a woman aged fifty-

eight, married, who dislocated her shoulder four months ago. The medical man who sent her writes: "She had a dislocation of the shoulder four months ago, which I reduced about an hour after it occurred, but I was suspicious of fracture or fissure about the neck of the humerus. She suffers pain in the hand and the area of distribution of the median nerve, probably due to pressure." That was on May 31st. The dislocation may have been reproduced since then; at any rate the bone is not in the socket now. Possibly the dislocation is four months old, possibly much less. She says she has not hurt it in any way since. I take it that the right thing to do would be in the first place to ascertain by means of the X rays, if we can get evidence of fracture about the neck of the humerus, and in the second place it might seem desirable to give her an anæsthetic to see what we can find out. If the Röntgen rays reveal a fracture, I do not think it would be uncharitable to imagine that the statement that the humerus was replaced was incorrect. There may have been at the time a great deal of swelling and effusion, which made the reduction of it monstrously difficult. I fancy it is not a case to manipulate very much now, as we might easily fracture the humerus or do a great deal of harm to a flabby stout woman of this description and at this age. An open operation would be safer and more satisfactory, and I am inclined to advise this if the pain is severe.

MR. BENNETT.—I certainly should not attempt any formal reduction of the dislocation at present in the ordinary way, and unless the pain the woman suffers is more than she can tolerate I should do nothing beyond getting as much freedom of movement as I could by rubbing and massage. If the pain increases so as to be intolerable, I should consider the question of an open operation supposing the patient is sound viscera, either for the purpose of reducing the head of the bone into the glenoid cavity, or with a view to excising the head of the bone in order to relieve the pressure upon the nerves which is probably the cause of her pain. I think any attempt at reducing the displacement without operation now would do more harm than good, and I do not believe for one moment that it would succeed. It is very rarely that one is able to reduce a dislocation of the shoulder by

ordinary means after four months, or even after a couple of months; I have seen grievous damage done by attempts of that sort, especially in fat people like this woman.

MR. HAWARD.—I agree in thinking that the condition of the limb is not very suitable now for the ordinary methods of reduction of the dislocation. I saw the woman a week or so ago in an informal way, and I thought then that probably the best thing would be to operate upon the joint if the pain were as great as she represented. She certainly said she had a great deal of pain and that it was increasing in severity. If that be so it would be justifiable to cut down upon the head of the bone. Her vessels seem pretty good, and there seems nothing at present to contra-indicate such a proceeding. Cases of this sort have done very well, especially after excision of the head of the bone, and they are not subjected to anything like the same danger that a violent attempt at reduction would involve.

MR. DENT.—I will do that if the woman will come in. I believe there is no fracture at all. Of course I shall take a photograph to make sure. As giving some indication of the pain, I may say that a fortnight ago she was loth to come in, but now she is anxious to do so.

MR. BENNETT.—Here is a man aged thirty-four, who is suffering from disease of the right femur, the condition having been going on since last November. Abscesses formed and were opened in the ordinary way; many successive abscesses have been dealt with by letting out the pus, but the disease appears to have been steadily progressive. He was admitted into the hospital on April 11th, at which time he had a large abscess occupying the greater part of the thigh, which was opened freely and drained. You will find that the femur from end to end is very thick and very hard. He has been in South Africa for many years, and you can see by the aspect of his face that he is the subject of some renal condition. He was at one time very pallid, but he is not quite so pallid now. He has a considerable amount of albumen in his urine, which recently has been increasing. He is not improving, indeed he is obviously going backward, as is shown by the increase of albumen in the urine, the specific gravity of which is very low. So far as I understand the case there is extensive osteo-

myelitis of the femur. It is unusual in severity, and unlike most of these cases which assume the form of popliteal necrosis, commonly so-called. There is no history of syphilis, but the patient has had gonorrhoea. He is now suffering much pain. The knee is stiff from adhesion round about it, but there is no disease of the joint. The hip moves and is healthy, although there is some slight stiffness from the muscular rigidity. The appearance of bony thickening is real, and not the result of solid oedema. So far as can be judged there is not yet any change of a lardaceous character in the viscera.

The question now arises as to the course of treatment to pursue. At the present time the temperature is not high, but the patient is slowly getting worse. Should the thigh be amputated; if so, at what point? Would any treatment short of this suffice to save the patient's life? On the whole I am disposed to think not. I therefore propose to amputate below the trochanter. Of course this is a strong measure in this patient's condition, but unless we can do something to relieve the strain upon his system which at present exists, he will go rapidly down hill and slip through our hands. I have no desire to urge too strongly a desperate measure of this sort, but of course I want to save the man's life if I can. I am prepared, if my colleagues think it wise, to make a further exploration and to make a freer examination of the bone, in order to see if there is a sequestrum, but I think the disease is too extensive for any hope from anything short of amputation.

MR. DENT.—Have you felt any dead or exposed bone?

MR. BENNETT.—Yes, through the sinuses rough bone is felt at almost any point you choose.

MR. DENT.—The point of the case is that this is a very recent affair if the history be correct. It appears that prior to November, 1899, there was nothing amiss. The condition looks like that of a tolerably acute disease of the femur, with osteo-myelitis and periostitis. It is highly probable that all that thickening of the bone is essentially an invagination of disease of the bone. If so, the albuminuria is a very grave matter indeed to my mind. If it had been a very much older disease, say four or five years, with evidence of sclerosis of the bone, then the albuminuria which

would be almost certainly present and very abundant indeed, would be far less serious, because if you got rid of the bone I think it is highly probable you would also get rid of the albuminuria. If you rid this patient of his trouble I am not sure you will get rid of the albuminuria. My view would be that I should first of all like to make quite clear that the disease was not entirely due to a sequestrum within the femur. In favour of that is the rather ovoid shape of the swelling. I think the only way to be sure of that is to expose the bone somewhere about the middle, and then trephine down into it; you would then be able to see what a section of the bone would look like, or at any rate examine it in some such way. If there were a sequestrum you would take that away. If it were found that there was a large amount of diseased bone or several sequestra so invaginated in the new periosteal bone that they could not be easily removed, I think the limb should be amputated. I think amputation might perfectly well be done below the trochanter, that is to say an inch or two below, and then I think the whole of the disease would be got safely away. The danger from such an operation is pretty formidable, still I think it would be right to do it. My view would be that you should explore first, and endeavour to ascertain whether you could deal with the sequestrum. I believe that to be unlikely; if such were the case, then amputate.

MR. HAWARD.—Although this man has a good deal of albumen, and it is increasing in quantity, I notice that the specific gravity of the urine is good, and I should think, probably, the albuminuria is not of very long duration. But the disease of the femur seems to be so very extensive that I doubt whether anything short of amputation will be desirable. I should myself advise amputation just below the trochanter, and I think that would be far the safest thing for the man's life. No doubt it would be reasonable to do what Mr. Dent suggests, namely, explore the femur first, but I understand that the femur has been explored to a certain extent, and that the finger passed down to the bone discovered no sign of any opening leading into a central cavity. There is one objection to extensive exploration, namely, that while you are doing that the man is losing a large amount of blood, and then if you go on to amputate afterwards the amount of shock will be

greatly increased. The bone is so extensively diseased that even if there be, as is quite likely, some necrosis, and not mere chronic inflammation, it would be hopeless to attempt to save the limb. I think it would be best to amputate.

MR. BENNETT.—I shall put the matter fairly to the man. The thing which appeals to me most strongly in favour of amputation is the excessive amount of albumen in his urine in proportion to the length of time the disease has existed. That, I think, rather puts out of court any prolonged attempt at getting healing, even if a sequestrum be got away. I do not want to sacrifice the limb if I can help it, but I am anxious to save the man's life.

CHAPTERS FROM THE TEACHING OF DR. G. V. POORE.

No. XXXVI.

GENTLEMEN,—In connection with death from fire there comes the question of *spontaneous combustion*; and it is a very interesting speculation as to whether such a thing ever occurs. I may state positively that I think there is no evidence whatever that a living human being ever spontaneously ignited, and I think it is quite unreasonable to suppose such a thing could happen. Spontaneous combustion does happen in nature; that is to say, things catch alight which have not been set fire to. Coal catches alight not only in a coal-mine, but in the bunker of a ship. One can only assume that in such cases a process of oxidation and heating goes forward, and that some inflammable gas catches alight at the temperature thus created. Cotton, in the same way, will ignite in the hold of a ship, and in engineering works heaps of cotton waste and oil have often been known to ignite. More remarkable than that is the ignition of a haystack. It is well known everywhere that if grass be cut wet and stacked wet the hayrick is very likely to ignite. Those of you who have ever examined a hayrick which has ignited will recognise the fact that the smell is quite peculiar. It is *sui generis*; it is not like burning hay, neither is it like the smell of a dung-heap. You will find that grass which ignites is the seat of a fungoid growth,

which growth takes place very rapidly. Just as the yeast plant in growing brews alcohol, so one must assume that the fungus which grows in wet grass brews an inflammable something—I cannot be more definite than that,—that the heat gets very great and the inflammable something ignites. It is remarkable how little exact knowledge there is upon this very important and frequent occurrence.

Is there such a thing as spontaneous combustion of the human body? I can no more believe in a living body igniting than I can believe in grass which is still growing catching alight before it is stacked. It is, however, perfectly certain that some human bodies are exceedingly inflammable, and I have read to you the account of a body in which the gases generated by decomposition proved to be strongly inflammable; and there is no difficulty, to my mind, in believing that gases may inflame spontaneously if the heat generated be sufficient to cause inflammation, using the term inflammation not in its pathological sense, but in its proper sense. We also know that the occasional rise of temperature in the dead body is accepted as an undoubted fact. So that there can be no doubt that some bodies are very inflammable, and I see no inherent impossibility in the spontaneous ignition of a body post mortem. Most of the cases of so-called spontaneous combustion have been in women, most of them in fat women, most of them in alcoholic women. Spontaneous combustion has been made use of by the novelist, and you will find an account of it in 'Jacob Faithful' or 'Bleak House.' Not only are such cases found in works of fiction, but I will give you one from the 'British Medical Journal' of April 21st, 1888, recorded by Dr. Mackenzie Booth. He records a case of apparently great combustibility in a drunken military pensioner. He was seen to enter a hayloft, drunk, on February 18th. A light was noticed through the skylight of the loft, which was also noticed to be extinguished subsequently. In the morning smoke was seen issuing through the roof, and the man was found dead and the body largely consumed. Dr. Booth found the remains reclining against the stone wall, and kept in position only by one of the joists and the burnt remnant of the flooring under him, which prevented him from falling through into the stable beneath. Combustible material, such as wood and straw, was near at hand, and not con-

sumed. That is a carefully recorded case, and the others are like unto it; and that is all I can say. With regard to spontaneous combustion in connection with the human body there has generally been a light present, and the subjects have very often been in a drunken slumber. There is a case recorded from France, and there have been similar ones reported, in which the body was found consumed, but not the muslin curtains near it. I think there can be no question that sometimes a body is in a condition in which it is likely to smoulder, and I think spontaneous combustion, if it occurs, is a thing which takes place post mortem and not ante mortem. I think also that it occurs in a fat, flabby drunken person who has been in a drunken slumber, in whom the cessation of life has been due to a fatty heart, the whole circumstances being favourable to the growth of a microbe which brews combustible gas or alcohol. I prefer to say "a combustible something" because, when we do not know what it is, it is much better to say so.

Very little is known of death from *heat and cold*. It is well known that people can stand enormous ranges of temperature, that man may continue to live at the Pole, that he continues to live at the tropics, and that the ranges of temperature which a man can bear in a Turkish bath are extreme. The causes of death from high temperature we do not know. Of course at certain temperatures you will get coagulation of the tissues, and then death may result. Death from cold is usually accompanied by numbness, torpor, and stupor, and it is well known that if the Alpine climber yields to the temptation to sleep he is done for. People stand cold just in proportion as they are fed; if a person is properly fed and clothed he can stand any degree of cold any length of time. It is the same with the lower animals.

The expression "starved by the cold" is a common one, and there is no doubt that death from starvation and death from cold are to a certain extent allied; but as to the post-mortem appearances which would enable you to say a person died of cold or of extremes of heat, I think I have not sufficient evidence to lay before you.

With regard to death from *starvation*, that is an important matter, and death from this cause is

divisible into two classes. First of all, cases in which solid food is withheld and water is obtainable; and next, cases where both food and drink are withheld,—that is to say, death from hunger and thirst combined. Death from the latter cause is very much more rapid than in cases where food alone is withheld. Death where water is withheld is probably death from uræmia, because the body is not flushed and the waste products are not carried away by the kidneys; and the person generally dies delirious, and often comatose, the body smelling very foul. Death from lack of food only is a totally different thing; and whereas a person will only live a week at the outside where both food and water are withheld, we know, as a result of music-hall experiments, that a man may go on living for six weeks with no food, or with very little food indeed. And I think that these music-hall experiments, which have been common of late years, have been useful—I wish to say so definitely. There may have been trickery in them. We know a man may get a very great deal of food in the guise of water; he may get colourless alcoholic food and syrups and albumen; and where water is given *ad libitum* a good deal of food may be incorporated with it. But it is certain that those people who have exhibited themselves for money and fasted for six weeks have lost a prodigious amount of weight,—that is to say, several stones. I think the exhibitions have been useful for this reason—that if people are put in circumstances where no food is obtainable, but water is obtainable, they may know that if they keep quiet they have a reasonable prospect of escape. Take, as an instance, entombed miners. In a mine there may be water but no food, and if it is known that by taking water and keeping quiet one may survive until rescued, it is a very important fact. One of the most interesting cases of death from starvation is the case of Sarah Jacobs, the Welsh fasting girl, and it is one of the few cases where both food and water have been withheld. The account is probably quite trustworthy. Public attention was first called to the case by the vicar of the parish in which she lived, who wrote to the 'Welshman' newspaper on February 19th, 1869, stating that Sarah Jacobs, who was twelve years of age, had "not partaken of a single grain of food of any kind whatever during the past sixteen

months. Medical men," he said, "persisted in saying the thing was impossible," but for his own part he fully believed in it, and he ended his letter by suggesting that a skilled and scientific investigation of the facts should be made.

After this letter there was of course endless discussion upon the subject. There were believers and infidels on the point, and amongst some of the superstitious peasantry the Welsh fasting girl became almost an object of veneration. She was visited by numbers of people of all classes, and her parents profited considerably by the "show" which they had started.

On December 9th all the arrangements for watching Sarah Jacobs were completed. A local medical man and four nurses from Guy's Hospital undertook the task, and we have in their evidence a detail of her daily symptoms. Sarah Jacobs was very hysterical, and seems to have had very slight left hemiplegia.

When the watching commenced she appeared, when naked, to be healthy, fat, and plump. The following official report was made by the watching committee:

"Thursday, Dec. 9th, 1869, Llethernoyadd.—Four nurses arrived at 2 p.m. The whole of the room in which Sarah Jacobs was lying was carefully examined; all the furniture carefully looked into; all contents of drawers removed; the bed on which the girl was lying was carefully examined, and each covering singly; the girl's body, clothes, and her hair were fully examined. Nothing of the nature of food (saving an old shrivelled-up turnip under the parents' bedstead) was found anywhere."

The medical report on December 9th was as follows: "Cheerful; face flushed; eyes brilliant; pulse regular, averaging 86 per minute; temperature in the mouth 98° after two minutes' rest. She has a warm water bottle at her feet. She seems quite well, and says she has no pain anywhere if not touched.

(Signed) "HENRY H. D., M.R.C.S.
THOMAS L., M.D.

Dec. 10th.—"Nochange. She continued cheerful and happy, and read aloud during the day. Pulse above 100.

Dec. 12th.—"During Sunday afternoon she was visited by the doctor, who made the following entry in the Sister's diary:—State of Sarah Jacobs

this day at four o'clock p.m.: Not so cheerful as when I last saw her before, face not flushed, but temperature of the mouth 98° after ten minutes' rest; pulse regular, averaging 112 per minute. She has not asked for anything, neither could she possibly have had anything without our knowledge. She has no pain whatever unless she is touched, but occasionally a little headache. She passed a quantity of urine; the head nurse thought her 'not so well,' eyes were sunk and cheeks flushed.

Dec. 13th.—"Passed urine, and the cork came out of the water-bottle, and the bed had to be changed. The opinion was that the girl was thirsty, and pulled the cork of the bottle out with her feet.

Dec. 14th.—"Had a true fainting fit. Pulse 112. Voice not so strong. Face very much flushed, lips dry. Passed a small quantity of urine. A peculiar smell was also noticed—'not like the usual smell of death.' It was not urine, nor yet from the feathers of the bed. She slept, but was very restless, and passed some more urine.

Dec. 15th.—"Her eyes were sunk in and her nose pinched. Her voice was much lower, and she was unable to read. Pulse 120; face not so flushed. A dusky redness of the cheeks and nose. She conversed pleasantly and coherently. Towards evening she awoke from her sleep and shivered a little. She was very restless, and the smell of her breath was very noticeable. This was the last day on which urine was found in the bed.

Thursday, Dec. 16th, 3 a.m.—"She was rolling from one side of the bed to the other. At half-past three she wished the bed made, and they made it. She was taken out of her bed, and the father having carried another bed in, the girl was lifted into it. Both parents, especially the father, were in and out of the room during this watch. Between 3 a.m. and 6 a.m. she was quieter. At this latter hour the Sister nurse found her looking very pale and anxious in the technical sense of anxiety. Her eyes were sunk and her nose pinched, and the cheek-bones more prominent. . . . Her arms and hands were cold, her feet and legs were the same. Very restless, and appeared to be sinking. . . . Her lips were very dry, and her mouth seemed parched. Friction to the right hand and hot flannels to the legs and feet made her 'much warmer.' She was, however, still restless, turning till 11 a.m., when she

got quieter. After that she was reading, but did not seem to care for it as she did before. This morning the 'peculiar smell about the bed' was so bad as to make the Sister nurse 'feel quite ill.' Towards the afternoon she began to talk to herself. The pulse was variable, rising as high as 160. That night she became delirious. The breath was still very offensive. She was not still two minutes the whole night, but kept calling out, etc. The nurse's scent-bottle was missed, and was found ultimately in the bed.

Dec. 17th.—At 2 p.m. the girl died, sinking gradually, and having been during the last hour of her life 'in a stupor,' and unable to be roused."

At the inquest J. T—, being sworn, said as follows:—"I am a surgeon in practice at N—. Yesterday, in conjunction with Mr. P— (Dr. L— being present) I examined the body of Sarah J—, said to be about 12½ years old. It measured about fifty-four inches, was plump and well formed, and showing indications of puberty. I opened the head, and found the membranes of the brain considerably injected; the substance of the brain was not very vascular, but perfectly healthy and of proper consistence, and there was no difference between the sides of the brain. An incision was made from the top of the girl's chest to the lower part of the body; there was displayed a fine layer of fat from half an inch to an inch thick along the incision; the contents of the chest—lungs, heart, and great vessels—were perfectly sound and healthy, and contained very little blood. On examining the alimentary canal I observed there was not the slightest obstruction from the mouth to the termination of the gut. The stomach contained about three teaspoonfuls of dark gelatinous fluid, having a slight acid reaction with litmus-paper. The small intestines were empty, but the colon and rectum contained about half a pound of excrement in a hard state. The liver was healthy, the gall-bladder considerably distended with bile, kidneys and spleen perfectly sound, urinary bladder empty. The body was perfectly free from disease, judging from the healthy appearance of the organs."

As soon as the girl was dead people were very angry indeed, and wanted to say that those who watched her and arranged for the fasting committed manslaughter. Certainly the ethical question involved is a very nice one. Your duty as a medical man is clearly this: you have no right to sit

by and see a person starve while there is such a thing as a stomach-tube. On the other hand, you may say the girl could have eaten if she had liked, and that the parents were consenting parties. There can be no question that the case was one of fraud. Fæces were found in the intestine, and they must have been evolved from something in the way of food. It was an unfortunate case altogether. It is of some use scientifically, because there is the daily record of a case of starvation, with the temperature, pulse rate, and so forth.

There has recently been a case of a woman who starved her servant girl to death, and got seven years' imprisonment as the result. In these cases you must be very careful to ascertain whether the emaciation, which is almost always present, could have been caused by disease. Examine very carefully for tuberculosis. Remember diabetes also. The defence put forward in the case to which I have just alluded was that the girl did not die of starvation, but of diabetes, which was answerable for the rapid wasting and the voracious appetite which was alleged. But there was no record in this case of the urine having been properly examined, and that defence was not accepted.

Now there are one or two matters which remain to be discussed in medical jurisprudence, one of which is *life assurance*. Life assurance is exceedingly common. People insure their lives with the object of making provision for their families, and very often also with the object of borrowing money, and the two objects are perhaps about equally common. There was some time ago a notable case before the public of fraudulent personation in connection with life assurance. When a man gets in the hands of money-lenders his downward descent seems to be rapid, and there seems to be nothing he will not stick at. It is very necessary to remember that in life assurance the medical officer is liable to be deceived—that is to say, intentionally deceived—by the proposer. Life assurance is a contract in which the underwriter for a certain annual sum promises to pay to the proposer's executors or representatives a lump sum at death. Now of course it is a well-known fact, which the statistics of the Registrar-General have made abundantly clear, that the death rate in a country like England is tolerably fixed, and that therefore the expectation of life at any particular

age is tolerably fixed also. For instance, the expectation of life of a male at one year of age is about forty years. At ten years of age the expectation of life is forty-seven years in a male ; it is rather greater in the female right away through :

At 20 the expectation of life in a male is 40 years.

30	"	"	33	"
40	"	"	27	"
50	"	"	19	"
60	"	"	14	"
70	"	"	9	"
80	"	"	5	"

These are actuarial facts, and are based upon an immense amount of statistics, and are worked out by mathematicians.

The first thing I would remind you about life assurance is that it is a matter of business, and that the statistics of life expectation are not theory, but facts. Life assurance offices would have no need to trouble in the least about medical examinations *provided everybody who passed the office door came in and insured*. If everybody in the country insured his or her life the offices would have no trouble at all, it would be simply a matter of calculation to find out what the premium should be. But everybody who insures his life is not sound. Very often people insure their lives because a "still small voice" within them tells them that they are not quite sound. A man goes on perfectly gaily until he perhaps, apropos of nothing in particular, staggers in the street, or faints, or feels giddy and queer; and then he thinks about his wife and children and the future generally, and proceeds to insure his life.

It is the same with a woman; she does not trouble about insuring her life as a rule, and insurance among women is less common than among men. It is a very interesting and remarkable fact that while the expectation of life is greater in women than in men, among *women who insure* it is less; all offices have proved that the insuring woman, age for age, is a greater risk to an office than an insuring man. Women rarely insure, because the provision for the family is a matter which usually rests with a man. The woman who insures very often has to bear the battle of life like a man, and cannot stand up against it so well. Moreover the woman who insures is often a person who has got into debt, and women of that kind are very "bad lives."

Life assurance offices employ the services of

medical examiners, and if they did not there is no doubt that they would be defrauded to a great extent. It is quite impossible to appraise the value of a doctor to an insurance office. A man has a house in the country, perhaps keeps a big mastiff, and on the door is the motto, *Cave canem*—beware of the dog. The owner of a house of that kind would be very wrong to assume that because he does not get burglarised the dog is of no use to him. He will never be able to tell how many intending burglars have put an eye over the fence and thought better about proceeding any further. In the same way, if there is a doctor to an insurance office the swindler as a rule gives it a wide berth. The first thing which a proposer has to state is his age, and of course that is a thing which the man has to prove; he must bring his birth certificate. I need hardly say that the amount of premium a man pays is in proportion to his age. For instance, a man wishes to insure for £100 at death—I take £100 as a convenient figure, but you may multiply it to any extent; and if that man is 20 or 22 years of age he will have to pay somewhere about £2 per annum. His expectation of life at that age being about thirty-eight years. He will pay some £76 in hard cash, and the accumulations of interest will be sufficient to pay the office and give his executors £100. If the candidate is 40 years of age he will have to pay more—about £3 5s. a year; if he is 50 he will pay about £4 11s.

The next point is the family history. If it is very bad, it means that the candidate should be "rated up." For instance, if there is a strong history of phthisis, and if the applicant be a young man with a delicate aspect, then, recognising that he is still young, and that the period when a man is most liable to tuberculisation has not yet passed, I should rate him up severely, or not accept him at all. If I accepted him I should probably add fifteen years to his age; that is to say, if he was 19 I should rate him as if he were 34, so that instead of paying £1 18s. a year he would have to pay £2 16s. If on examining such a subject I found the slightest indication of anything wrong with the lungs—such as a faulty percussion note at one apex—I would not have him at all. But if the applicant was a sailor aged about 35, or an officer in the army of that age who had been through a campaign, and showed no signs of tuberculosis,

then, even if the family history was not good, I should consider him an average risk and take him at the ordinary rates. In life assurance, in all its bearings, you have to use your common sense. There are other hereditary diseases, such as cancer, diabetes, insanity, and so forth, and these have to be taken into consideration. But much more important than the family history are the *habits*; indeed, there is nothing so important, from a life insurance point of view, as habits. If you are satisfied that a man is intemperate, it is better to go without him; you cannot appraise his life at all; he is always a very risky life. Of course you examine a candidate carefully, especially the heart and lungs; and we cannot lay down any rule with regard to these cases. But I should say that if he has any valvular disease of his heart—I am rather peculiar in that—I would not have him at all. Of course one must be exceedingly careful in the matter of lung or visceral disease. Then comes the question of urine. Permanent albuminuria affects the expectation at different periods of life generally. If you find a young man with permanent albuminuria and an hypertrophied heart you would not take him, especially if he was thin. If, however, the albuminuria was only transient you would have to consider the matter otherwise.

Now with regard to the examination of the urine, which is a very important thing, you must be sure that you are not being deceived; and people do try to deceive you in life assurance matters. I remember a man coming in to insure his life. He was very smart, and had on a big fur coat. I said to him, "Will you pass me a little urine into this vessel?" I sat at a table writing, and I noticed that the sound of his passing the urine was very odd; it was a succession of pops like the sound made by water coming out of a narrow-necked bottle. I looked up from where I was writing, and I could not see what he was doing because of the big coat. But as he finished I saw his hand go into his side pocket. You have always to be looking out for fraud, and it crossed my mind that the urine was not his own. But, on the other hand, it would be very awkward to make a mistake; it would be very awkward to wrongly accuse a man of fraud of that kind. I confess I was in very considerable doubt as to what to do until he put the glass beaker into my hand.

Directly I got it into my hand I knew what to do. I said, "We cannot take you; you must be exceedingly ill; this urine is as thick as mustard and as cold as ice." We found he had been trying to deceive the people at another office. He had brought somebody else's urine in a bottle. There was no albumen in the urine he gave me, but he had been rejected elsewhere for albuminuria, and some of his urine obtained subsequently was found to be albuminous. One of your duties in life assurance is to take the height and complexion of the applicant and see him sign his name, so as to prevent personation as far as possible. It requires a very great deal of practice to know how to deal with some of these cases; a young man who examines for life assurance has his education to get in this matter. Remember that if you are fidgety and reject candidates for trifles you will be keeping away valuable business from the office. You have to reject at once really bad lives about which there is no question; but it is a very fine line which divides proper caution from an improper fidgetiness, and I do not think I could do any good by endeavouring to lay down rules for your guidance; you will require to serve a considerable apprenticeship before you can justly appraise these things. But I warn you to be careful and look out for fraud.

Carcinoma.—The method of Dr. Massey consists in cataphoric diffusion of the nascent mercuric salts, produced by electrolysis of metallic mercury inserted by the gold anode into the growth, the patient usually being under general anæsthesia, and the chemical and cataphoric force being a direct current of 200 to 1200 milliampères continuously employed for a time varying from fifteen minutes to two and a half hours. The immediate effect is the production of an area of necrosis involving the growth, beyond which extends a zone of sterilisation in which the malignant germs are killed without destruction to the normal tissue elements. He claims that he has employed this in thirty-seven cases, with good results in thirteen.

Journ. Amer. Med. Assoc., April 21st, 1900.

"Light Treatment" of Lupus.—Mr. Malcolm Morris gave a demonstration last Saturday at his "Studio," 1A, Berkeley Gardens, Campden Hill, on the cases he had had under treatment for the last two months.

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TWO CLINICAL LECTURES

ON THE

CONSTRUCTION

AND

ADJUSTMENT OF TRUSSES.

Delivered at the West London Post-Graduate College,
May 21st and 24th, 1900,

By W. McADAM ECCLES, M.S., F.R.C.S.

Assistant Surgeon, West London Hospital, and City of
London Truss Society; Examiner in Anatomy to
the Society of Apothecaries, etc.

LECTURE I.

The Construction and Adjustment of Trusses for Reducible Inguinal Hernia.

GENTLEMEN,—I purpose to give two short demonstrations on the application of trusses, but in order that we may fully comprehend the adjustment of a truss it is, I think, requisite that we should have some knowledge of its construction, so that its different parts may be understood in their relationship to the structures to which they are applied. I will, for this purpose, take an ordinary inguinal truss as a pattern.

Such a truss consists practically of three parts: the pad, the spring, and the straps. The pad differs in shape according to the variety of hernia that it has to control. The shape of the pad of an ordinary inguinal truss is somewhat oval, with its long axis horizontal, and measuring about three and a half inches, its depth being about one and a half inches, and its thickness about three quarters to one inch. It is slightly convex posteriorly, and has on its front aspect two buttons or studs. The basis of the pad consists of metal, as you will see in this dissected truss (see Fig. 3). As a rule the metal used is soft iron, whereas the metal of the spring is steel, a point which is of some importance. Attached to the metal pad posteriorly there are two sharp points, and against these rests the substance which is to form the convexity of the pad. This may consist of various

materials, but, as a rule, a piece of shaped cork is the most satisfactory. The cork, you will observe, is enclosed by two layers of house-flannel or felt; and lastly, the surface which comes in contact with the skin is covered with ordinary chamois leather. The other materials which may compose the pad are celluloid, ebonite, wood, or india rubber containing air or glycerine. But so far as I know from practical experience, no pad is so comfortable and so uniformly useful as the pad which I have described. Naturally, if the patient has to reside in a tropical climate, or his occupation causes considerable perspiration, it is advisable to wear a pad which does not absorb moisture, though this is not so comfortable as one constructed with a leather facing.

We next come to a very important part of the truss, namely, the spring. There are three varieties of spring in ordinary use. The first is a spring contained wholly in the pad itself. You are all probably acquainted with what is known as the moc-main truss (see Fig. 1). You will

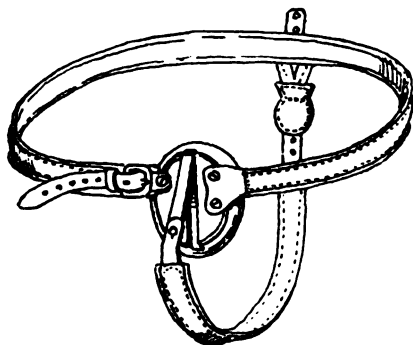


Fig. 1.—A moc-main truss.

remember it consists of a circle of leather, having no spring in this portion, but in the pad itself there is a small spring which fits into a groove or slot. Pressure is made through it by the application of the under-strap. This form of truss with the spring wholly in the pad is said by a great many wearers to be extremely comfortable, and in certain cases of slight hernia may be efficient; but in by far the larger number of inguinal herniæ it will only afford a false security. As a rule, therefore, it is not a truss that can be recommended.

The next variety of spring, of which this is a specimen, is semicircular, or a little more than a

semicircle. It has been known by the name of the Salmon and Ody pattern. In the form of truss having such a spring (see Fig. 2), there is



Fig. 2.—A truss with semicircular spring.

a pad behind and a pad in front, with the semicircular spring extending between the two. It often requires a strap on the opposite side to keep it in position. There are serious objections to this form of truss. It is extremely liable to become shifted during the movements of the wearer. It is also likely to fail in maintaining pressure in the direction in which it is wanted; and, as a rule, the pad behind causes a considerable amount of annoyance. Still there are cases in which a Salmon and Ody's truss is a suitable appliance to use.

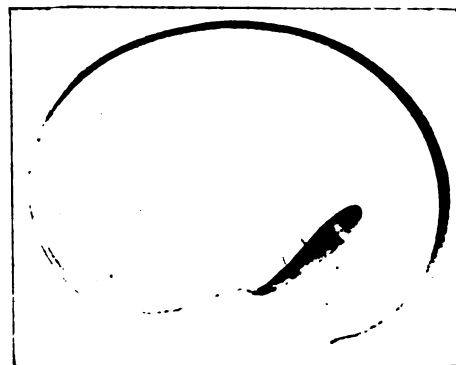


Fig. 3.—A spring which is more than a semicircle.

Lastly, there is the spring which is more than a semicircle, and in a single truss such a spring will extend from the pad on one side to just in front of the anterior superior spine on the opposite side. I consider this to be the best variety of spring for ordinary use. There are, however, one or two minor details in its construction which

make all the difference between comfort and discomfort to the wearer. In the first place the spring should have not less than a certain breadth; three quarters of an inch is none too broad. Secondly, the spring should have its upper border the arc of a smaller circle than its lower border. This is obviously necessary if you remember the obliquity of the parts to which it has to be applied. If both borders were the arc of the same circle, the spring would rest only on the lower border and give rise to great uneasiness. Thirdly, the end of the spring should be hammered out so as to make it quite pliable, and therefore easy of adjustment to the patient's body. If you look at the termination of this spring you will find it is so hammered out as to be thinner, and therefore more flexible than the rest of the spring. You will be able to frequently determine by this small detail alone the difference between a well-made truss and an imperfectly fashioned one.

The attachment of the pad to the spring is also an essential detail. For the comfort of the wearer and for the efficiency of the truss, it is important that the pad should be immovably fixed. Many trusses have their pads movable upon the spring. You see that in the Salmon and Ody truss which I have passed round. But, as a rule, such an attachment causes a certain amount of discomfort, and, in many cases, a considerable amount of inefficiency.

Finally, there is the covering of the spring. If you look at this dissected truss you will see the materials consist first of all of a piece of oiled paper or oiled linen, which is applied directly to the metal in order to prevent, as far as possible, the perspiration of the body attacking the steel of the spring. There has not yet been found any substance suitable for a spring which will resist the rusting or corroding action of sweat, and if such a material were found it would be a boon to truss-wearers. Next to this oiled paper you will find there are two thicknesses of felt or house-flannel, and outside this, covering the surface next the skin, chamois leather. The coverings of the spring should be full, and should pass beyond the breadth of the spring for at least a quarter of an inch on either side.

Now I pass to the straps. There are two straps, both of which are essential points of a

truss if it is efficient and comfortable. The cross-strap is a prolongation, in a single truss, of the leather that covers the spring, and passes to the



Fig. 4.—An ordinary inguinal truss.

upper stud on the pad. In a double truss it is a separate piece of leather, which fastens to the upper button on the face of each pad. The under-strap, sometimes called the perineal strap, or thigh-strap, is that part of the truss which is perhaps most frequently omitted or badly adjusted.

Let us look at the use of the under-strap. The purpose of the under-strap is to draw the side of the spring down from off the crest of the ilium, and at the same time to somewhat face the pad up, so that it may act more efficiently. I show you what I mean on this patient. He has an ordinary inguinal hernia, and you will notice the position of the under-strap. It is attached to the truss a little behind the shoulder of the truss; the shoulder of the truss is where the spring begins to bend back, and just posterior to this is the place where the under-strap should be attached. It is usual in the representations of trusses to find the under-strap is figured placed at the back of the spring, and that is where it is often put by patients. But in this position its objects are entirely defeated. The under-strap should be fixed and yet movable. It should be so fixed that it cannot be shifted by the movements of the patient, and yet so adjusted that the surgeon may move it, if it is necessary, half an inch to an inch further back. Notice that the under-strap draws the spring away from the crest, then it is adjusted round the outer side of the

thigh, along the gluteal groove, and up to the front, to be attached to the lower button on the face of the pad. There is no need whatever for the under-strap to be tight; it should do its work without causing any discomfort if it is rightly applied. The use and position of the under-strap is therefore a very important detail.

A word or two about an ordinary double inguinal truss. The pads are the same, each pad being like its fellow. The spring, however, is pro-



Fig. 5.—A double ordinary inguinal truss.

longed from one pad to the other, and is therefore almost a complete circle, open in front. Between the two upper studs on the front of the pads should stretch the cross-strap, the under- straps again fitting on to the studs at the lower part of the pads.

I now want to show you practically the best way in which to measure for a truss. To accomplish this the patient should always be lying down. The surgeon should stand on the patient's right, and taking a flexible draper's measure, he slips the tape underneath the patient's body from the patient's left allowing the figures on the tape to be in contact with the patient's skin, the lower numbers to the patient's right and the higher on the patient's left. Placing his left thumb at the commencement of the numbers, he brings the tape round so that it lies over the site of the deep abdominal ring on the patient's right side. The other part of the tape he takes in his right hand, with his right thumb against the figures, and brings the two thumbs together, drawing the tape fairly tightly. Then dropping the measure where it is held by the left hand, and maintaining the hold on the tape with his right thumb, he pulls

the tape away from the patient and reads off the number at the position of his thumb. The length thus obtained is that required for an ordinary single truss. If you have to order a double truss it is well to add one inch to the measure-

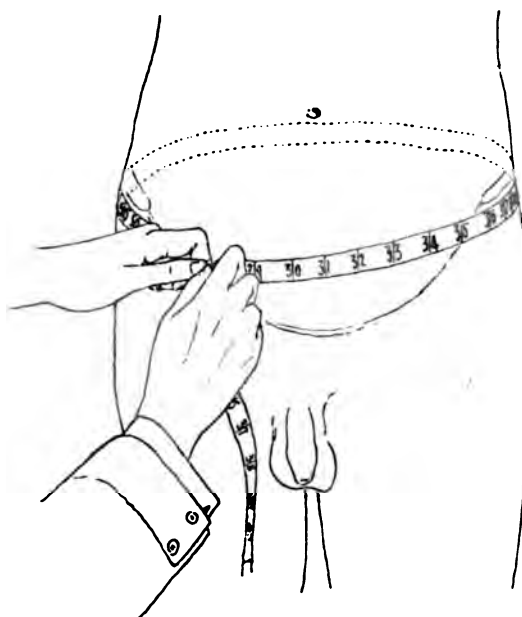


Fig. 6.—How to measure for an inguinal or a femoral truss.

ment, because a double truss does not stretch at all, whereas the cross-strap of a single one stretches considerably. It is important to remember that your measure should be at the base of the sacrum behind, half way between the crest of the ilium and the top of the great trochanter at the side, and at a higher level than the symphysis pubis in front. In ordering a truss from a truss maker, he will require to know, the variety of truss—whether it is inguinal, femoral, or umbilical; the size of the truss, that is to say the number in inches which your measurement gave; further, if it is a single truss, the side for which it is required. Lastly, a note should be made of any other special point, whether you want a particular shape or variety of pad, or the springs stronger than usual.

Now we come to the adjustment of an ordinary single inguinal truss. The site of the deep (internal) abdominal ring is half way along Poupart's ligament and one finger's breadth above the liga-

ment. The inguinal canal runs downwards and inwards from that point for an extent of one and a half inches in the adult, and terminates in the superficial (external) abdominal ring, an opening which you will remember is usually triangular. In order to efficiently control an ordinary oblique inguinal hernia one must place the pad in such

touch the outer edge of the rectus abdominis muscle in an ordinary oblique inguinal hernia. The common place for an inguinal pad to be placed, generally by the patient, but sometimes, I am afraid, by the surgeon, is too low down and too internal. If it is thus improperly adjusted, the result will be that, in the male, the spermatic



Fig. 7.—Adjustment of single ordinary inguinal.

a position that pressure is brought to bear over the site of the deep ring and the length of the canal. The spring comes round below the anterior superior spine, and the pad lies over the deep ring and the canal and does not impinge upon the superficial ring. The pad should just

cord, which is a most delicate and sensitive structure, is pressed against the body of the os pubis, causing the patient so much discomfort that he desires to discard the truss. There is no reason why a properly fitting and properly adjusted truss should give rise to annoyance. The symphysis

pubis is at a higher level than it is generally thought to be. The whole of the inguinal canal from the deep ring to the superficial ring lies above the level of the symphysis pubis, therefore the whole pad should be above that level. A truss should never be adjusted with the patient standing up. Patients should be instructed to put on their trusses before rising in the morning, first the washable truss for their bath, and then, taking off that one while recumbent, adjusting another for the day's wear. When the surgeon applies an inguinal truss for a patient, he should take it in his right hand, with the back of the spring facing the patient's head, a very important detail. The patient is then instructed to raise the pelvis, and the surgeon slips the truss under it from the patient's left, seizes the truss with his own left hand, and draws it round the pelvis, when it will spontaneously fall into position. Then the cross-strap is fastened, and its point of attachment to the stud acting practically as a pivot, the under-strap is drawn upon so as to pull the spring away from the crest of the ilium, and at the same time to make the pad face a little upwards. The under-strap is now slipped round the thigh and made to fasten on the lower stud on the face of the pad. Care must be taken not to include or pull upon any pubic hair. When thus properly adjusted, pressure upon the pad will prove that it is resting entirely upon soft parts. If it was over the bone it would press the spermatic cord against the body of the os pubis.

In a certain number of cases you will find that an ordinary inguinal pad will not retain the protrusion—generally in instances where the hernia has been neglected—then it becomes necessary to use what is called a scrotal or rat-tailed truss. Now it is important to get this truss of right construction. I show you a properly formed rat-tailed truss. You will see the iron of the pad has precisely the same extent as that of an ordinary inguinal pad. The tail portion consists wholly of soft material. Here is an improper rat-tailed truss with the iron prolonged into the tail. This prolongation of the unyielding part of the pad must give rise to pressure upon the spermatic cord to such a degree that it is unbearable.

Another very important point is that the under-strap or continuation of the tail should fasten to a

fixed stud just behind the shoulder of the truss. You will notice that in this improperly constructed truss the tail is made to fasten to a movable buckle, which the patient invariably places at the posterior part of the truss. Another point is



Fig. 8.—A rat-tail truss of proper pattern.

that the pad should have what is called a crank upon it, that is to say, it should be at a level which is somewhat anterior to that of the spring, and should be made to face somewhat upwards. If you look at this badly constructed truss you



Fig. 9.—A rat-tail truss of improper pattern.

will find that the pad practically continues the curve of the spring, and that the pad has its face almost directly antero-posterior, whereas if you look at a properly constructed rat-tailed truss you will find the pad is at the anterior plane to the spring, and that it is made to look face up-

wards. Because when a patient is standing upright, especially in a subject who is somewhat fat and has a large inguinal hernia, the abdominal rings are more horizontal than antero-posterior.

The method of measurement for the rat-tail is exactly as for an ordinary inguinal truss, except that in many instances it is better to take

fork between the scrotum and the thigh. The tail is then taken round the gluteal fold and fastened to the immovable hook close behind the shoulder of the truss. In this way the iron of the pad rests as before against the soft parts of the abdominal wall, and the soft tail portion lies in front of the body of the os pubis, but



Fig. 10.—A double forked-tongue truss adjusted.

off one inch from the actual measurement, for the reason that the cross-strap and tail-strap stretch so much.

It is adjusted in the same way as an ordinary truss. The cross-strap is first fastened, and then the pad should be lifted by means of the under-strap, so that the tail fits comfortably into the

being only of soft material produces no excessive pressure upon the spermatic cord. If the iron of the pad is prolonged into the tail, as it so commonly is, then there would be improper pressure, and the patient would complain bitterly. In a very few days the soft tail of a proper truss will mould itself so as to fall exactly into position.

If the pull on the pad by the tail portion tends to drag the pad proper from off its position over the canal, then the inguinal hernia may chance to slip out at the upper and inner part of the pad. To overcome this a forked-tongued truss may be employed. The difference in the construction of this from the rat-tail is that from



Fig. 11.—A single forked-tongued truss.

the upper and inner angle of the pad a piece is prolonged, which terminates in a strap to be fastened to a fixed buckle at the shoulder of the opposite side. In adjusting, first the cross-strap goes to the stud on the face, then the forked-tongue is carried above the cross-strap and fast-



Fig. 12.—Double forked-tongued truss.

ened to the buckle on the opposite side, and lastly, the under tail-strap is carried round and fastened to the stud at the shoulder on the same side. Whilst the tail-strap tends to pull the pad off the inguinal canal, the forked-tongue portion keeps it up in position, and so prevents the hernia

from prolapsing at the upper and inner part of the pad.

In cases where there is a scrotal hernia on one side and an incomplete inguinal hernia on the other side, it is usually advisable to give the patient a double forked-tongue truss, because if a forked-tongue on one side and an ordinary inguinal pad on the other side is used, it is not symmetrical and tends to give the patient discomfort. You will notice the forked-tongue piece is fastened to the forked-tongue piece on the opposite side by a webbing cross-strap, and in addition there is the ordinary cross-strap of a double inguinal truss; the tail portion is prolonged into the strap which fastens to the fixed stud at the shoulder of the same side. This is a most useful and efficient truss (Fig. 10).

(The Illustrations used are from '*Hernia: its Ætiology, Symptoms, and Treatment.*' By the Author.)

TWO CLINICAL LECTURES ON LEUCOCYTHÆMIA.

Delivered at the London Hospital,

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LECTURE II.

FOR clinical purposes we may distinguish acute and chronic leucocythæmia. And I may say at once that the acute form is a very rare one, few cases of it having been described.* Most of the recorded cases present somewhat similar features, that is to say, they are cases in which the increase of leucocytes is almost entirely represented by lymphocytes, so much so that some authorities propose to give to this form the name lymphocythæmia. In the commoner chronic form the disease is usually of the spleno-medullary type, though such cases occasionally take a rapid course.

* I was ignorant at the time this lecture was delivered of the interesting paper on "Five Cases of Acute Leukæmia," by Dr. Rose Bradford and Dr. Batty Shaw ('Med.-Chir. Trans.', 1898), which contains much valuable information on this subject. I have recently had under my care another case of acute lymphatic leucocythæmia in a boy aged twelve years.

I may now give you a brief abstract of the main clinical points which are to be noted in a case of leucocythæmia, before I pass on to the pathology of the disease.

It is a disease which begins in all cases more or less insidiously; it is very difficult to mark the date of onset. The patients complain of debility, loss of flesh and strength, and very frequently of pain in the left side. In these cases we shall generally find the spleen markedly enlarged. In other cases enlargement of the abdomen is one of the first points that attract the attention of the patient. The degree of anæmia or the pallor of the patient is very variable. In some cases it is a marked feature from the first; in other instances the amount of anæmia, as judged by the pallor, is not marked. Most of the symptoms from which these patients suffer are the result of the condition of the blood—of the anæmia. Here I must say a few words about the circulatory organs. Very frequently the heart shows a certain amount of dilatation, and presents the characters which we find in ordinary anæmia; the dulness is increased, chiefly upwards and to the left side, and hæmic murmurs may be present. The pulse is rapid and of low tension. Microscopically the blood shows the characters to which I have already alluded.

Passing now to the other organs, we sometimes find that the patients suffer from vomiting or from diarrhœa, but these gastro-intestinal symptoms are not constant. In some cases there may be hæmorrhage from the stomach and the blood may be vomited, as was the case in the child whom I have mentioned. But in that case there was also oozing of blood from the gums, and therefore it was difficult to say how far the blood came from the stomach originally, and how much of it was swallowed. It may have been gastric, as post mortem we found submucous hæmorrhages in the stomach. Ascites is a symptom which has been noted by some observers, but I have not seen it myself. General peritonitis is an occasional symptom. Localised peritonitis over the spleen—perisplenitis—is very common, and occurs chiefly where the spleen is greatly enlarged. The nervous system has been known to be affected in many instances, but I cannot say that I have had experience of it myself. The symptoms described are headache, giddiness, and sometimes

coma resulting from hæmorrhage into the substance of the brain. The retina is another organ which is sometimes affected, the lesions consisting of whitish patches, not unlike what we see in albuminuric retinitis. Purpuric eruptions are common in the acute cases (as in Case 2, Mabel M—), and occur towards the close of the chronic or ordinary spleno-medullary form. These hæmorrhages are mostly like ordinary purpura; indeed, some of these cases have been mistaken during life for hæmorrhagic purpura. Examination of the blood, however, will prevent such an error of diagnosis.

There is another curious and occasional form of hæmorrhage into the skin, of which I have seen no description in books, and an instance of which I saw many years ago, when I was house physician under the late Dr. James Andrew, in an elderly man, the subject of spleno-medullary leucocythæmia. A large swelling appeared in one axilla and rapidly fluctuated. Incision gave vent to a quantity of brownish semi-fluid blood. The patient lived a few months after this was opened, and the wound gave no further trouble. There are many diseases accompanied by profound anæmia in which hæmorrhage takes place into different parts of the body. I may mention the case of scurvy, which is now a comparatively rare disease in England. In two or three instances I remember hæmorrhage occurring into the subcutaneous tissue—large blood-tumours being formed. The urine in leucocythæmia often presents certain alterations. Uric acid has been found in excess in a considerable number of cases, and you will remember that in one of the cases which I mentioned (No. 3) uric acid crystals were frequently present in the urine, and uric acid calculi were impacted in both kidneys. Moreover, many observers, long before any theory was advanced as to the origin of the uric acid, discovered that in the urine of leucocythæmic patients there was an excess of xanthine and similar bodies. We shall see the importance of that directly. One case illustrated that rather rare complication, priapism, which I alluded to in the last lecture.

With regard to the spleen there is nothing special to be said which has not been illustrated already. The spleen shows enlargement, it is very firm and hard-edged, and the notch can be

easily felt; it may reach the enormous size of five or six pounds, which is not by any means unusual in the spleno-medullary form. The lymphatic glands are generally enlarged, but it is unusual to find any extreme enlargement in the spleno-medullary variety. Persistent pyrexia, mostly of a remittent character, is a frequent symptom. The explanation of this is still uncertain, but when we get a full grasp of the pathology of the disease we shall doubtless be able to understand the cause of the pyrexia.

Diagnosis.—In an average case of the spleno-medullary form the diagnosis is perfectly simple; the anæmia of the patient, the large spleen, and the marked increase of leucocytes in the blood are quite enough to prevent any mistake. But it is important to remember that cases have been recorded by Dr. Osler and others where the number of leucocytes fell to the normal at a certain stage of the disease. Accordingly, we must not be satisfied with one examination of the blood in suspected cases. The cases presenting most difficulty are those where there is no great enlargement of the spleen or lymphatic glands, and where there is a comparatively slight increase of leucocytes; and that leads me to consider what is meant by leucocythæmia and what is meant by leucocytosis. A few years ago it was said that leucocythæmia was represented by a very great increase of leucocytes, where the proportion of white to red cells was one to ten, or higher. It is quite clear that any such proportion is insufficient for purposes of diagnosis, because there may be no increase of leucocytes during certain phases of the disease. Nowadays the diagnosis turns not merely on the number, but on the type of leucocytes. In the common form of leucocytosis, found in cases of suppuration and in certain specific fevers, the increase concerns mainly or exclusively the polynuclear cells. But even now it is not quite certain, especially in the case of young children, whether a moderate degree of leucocytosis, in which the lymphocytes are increased, is sufficient to justify a diagnosis of leucocythæmia, and if I have time I hope to bring before you three cases which bear upon this point.

We may say, then, that the diagnosis of leucocythæmia has to be made practically by examination of the blood with the microscope.

With regard to the prognosis there is not much to be said. As far as we know all cases of leucocythæmia end fatally. But it is of some importance to know which cases are likely to live longest, and which are likely to run a short course. Generally speaking the spleno-medullary form runs a chronic course, but there are cases in which the progress has been rapid. It may last one, two, or even three years after it is recognised. In most of the small number of cases of lymphocythæmia which have been recorded the course has been more or less acute. Once hæmorrhages begin to appear, no matter in what form of the disease, a serious condition of blood is betrayed, and the patient's days are not likely to be long.

The treatment is most unsatisfactory; the only drug which has any influence on the disease is arsenic, and in most cases arsenic has proved very disappointing. It has been proposed to give medulla of bone, and some even advise extract of spleen, but as far as I know the results of such treatment are not encouraging. In one case that I can remember the use of turpentine seemed to be attended with considerable advantage lasting for some months.

I must now turn to the pathology of the disease. First of all a few words as to the pathological anatomy. The condition of the blood has been variously described by different authors. It has been described as thin and watery, or yellowish-looking; but in some cases it shows none of these characters, and I have not seen this appearance myself. In the case of the heart, it has been noticed in some instances that the clots which form are very large and gelatinous, but that is by no means the rule, judging from my own experience. In regard to visceral lesions, the enlargement of the spleen seems to be due to infiltration of the pulp with leucocytes. Changes in the spleen seem to be comparatively slight compared with the very gross enlargement of the organ. I have here sections of the spleen from cases of leucocythæmia, which you can look at for yourselves. I have not been able to find any change in the structure itself, except in the number of leucocytes which infiltrate the organ. In some cases there is a certain amount of thickening of the fibrous trabeculæ, but there is no reason to believe that this is essentially related to the disease. It is not the cause of the disease, probably

it is the result of the great enlargement and distension of the organ. Infarcts are very common, as in one case which I described last time, and they are believed to be due to thrombosis taking place in the small vessels as a result of accumulation of leucocytes—leucocyte thrombosis, as it is called.

Next with regard to the bone marrow, about which you have heard so often. Attention was first directed to the bone marrow by Neumann, and he described two varieties of lesion:—(1) the lymphoid, (2) the pyoid form. The second form is extremely rare, and I have no experience of it at all. In this form the medullary tissue looks yellowish, soft, and puriform. In the other form, which is common, the marrow is pinkish, and microscopically it is found to consist largely of dense aggregations of leucocytes, filling up the medullary spaces, in addition to the ordinary constituents of bone marrow, myelocytes, eosinophile cells, nucleated red corpuscles, large cells containing red corpuscles, and giant-cells. In the lymphatic glands the changes are seldom very pronounced, but it seems as if the enlargement were attributable to accumulation of leucocytes of various forms in the gland tissue. Some writers describe marked enlargement of the thymus gland. I have no experience of it myself, but the enlargement seems to be due to leucocyte infiltration. The liver is almost constantly enlarged, and can generally be felt during life. One case which I described last time in which the liver was much enlarged presented a not uncommon appearance. It showed on section a number of whitish irregular markings, and one of the sections under the microscope will show you that these whitish patches are due to an accumulation of leucocytes filling up the portal spaces. Sometimes you may find a more extensive infiltration, the leucocytes penetrating between the columns of liver cells. In certain cases the alimentary canal has been found to show very marked increase of the lymphoid tissue which is so widely distributed in that system from the mouth to the anus. This condition, however, is not constant. The tonsils may also show considerable enlargement. In the kidneys we find very much the same sort of changes as in the liver—a leucocyte infiltration, mostly in the cortex; but in one specimen which I show you the whole of the kidney is simply stuffed

with leucocytes, so that one can hardly trace any kidney structure at all. In some cases the tubular epithelium shows fatty changes. I have not seen anything of this sort myself. In the case of the lungs, some authors describe a peribronchial accumulation of leucocytes; of that again I have had no experience. Hæmorrhages are, as I have often had occasion to mention, very frequently found in various parts of the body, and, as we usually find to be the case, the serous membranes are markedly affected. The brain is sometimes the seat of hæmorrhages which have been attributed to leucocyte thrombosis occurring in small veins.

The changes which are found in the retina are due to the accumulation of lymphoid cells.

Pathological Chemistry.—It has been found that the fibrin is increased, but, contrary to what we should expect, the coagulability of the blood is diminished. It was asserted by one author that this was the result of peptone in blood, but of this there is some doubt. The xanthine bases are increased, and certain organic acids have been found in the blood—lactic, succinic, and formic acid. Charcot's crystals are commonly present in the blood after death, as well as in the spleen and bone marrow. Uric acid is not found in the blood, but large quantities are excreted by the kidney. The increased formation of the xanthine bases and of uric acid has been attributed by Kossel and Horbaczewski to regressive changes in the leucocytes.

Ætiology.—Among the predisposing causes which are commonly given, age is perhaps of some importance,—that is to say, leucocythæmia is more common in early middle life; but experience is accumulating to show that the disease may occur during the first few months of life. It seems to be more common in the male than in the female sex, and, as a rule, we find no hereditary influence at work. In Case 2, according to the history given by the child's mother, it is possible that the father was the subject of leucocythæmia himself. But even so, it is probably only an accidental relation, for, as a rule, no hereditary influence can be traced. Still, it is an interesting point, and perhaps worthy of record. Among other predisposing causes syphilis, rickets, and rheumatism have been present in individual cases, but it is very doubtful whether any of these dis-

eases have any relation to leucocythæmia. It seems that leucocythæmia is not peculiar to the human subject, but has been found in the lower animals. It is said to be not uncommon in the dog, cat, sheep, ox, pig, and some other animals.

As to the nature of this disease we are still more or less in the dark. Dr. Muir, in his excellent article in Allbutt's 'System of Medicine,' suggests that in the case of the lymphatic form there are very close analogies to sarcomata, in which certain cells take on excessive growth and overrun various parts of the body. He would even go so far as to assert that the anæmia which accompanies this form is due to choking of the blood-forming tissues of the marrow with leucocytes. I cannot say that this seems altogether a satisfactory explanation. As Muir points out, we may, provisionally, regard the lymphatic form as a disease in which there is an excessive production of lymphocytes, and knowing, as we do, the immense area over which lymphoid tissue is spread in the body, there can be no difficulty in explaining the production of such an enormous number of lymphoid cells. In the spleno-medullary form the essential element appears to be an excessive production of myelocytes in the marrow of bone, and Muir suggests that these cells make their way into the general circulation. What the anæmia is due to in this case Dr. Muir does not state. It is fairly certain now that the spleen is not the primary seat of the disease, as Virchow originally believed. Enlargement of the spleen is one of the most striking features, and the natural suggestion would be that the spleen is the chief seat of the disease. But, as Muir points out, the fact of any particular organ undergoing great enlargement is no proof that that organ is the point of departure of any particular disease. This is well seen in the case of ague, typhoid fever, and acute tuberculosis, in which the swelling of the spleen is admittedly a secondary occurrence. Whether the splenic pulp contains definite blood-channels lined with endothelium, or whether it is simply provided with a loose vascular network without any true lining membrane, it is certain that the relation of the circulating blood to the pulp is very intimate. There is reason to believe that leucocytes, under normal conditions, undergo disintegration in the spleen. There is still some doubt as to what the destiny

of the leucocytes is. The question of the transformation of leucocytes into red corpuscles I think is settled. Leucocytes do not give rise to red corpuscles; red corpuscles are developed from certain special cells found in the bone marrow, the erythroblasts. Another point of interest is, what is the relation of the various forms of leucocytes to one another? Are the myelocytes, the lymphocytes, and the other leucocytes essentially distinct cells, or are they modifications of some primitive cell? That is a point which has yet to be decided. It seems that the chemical changes found in this disease, especially the increased production of uric acid, of xanthine, and similar bodies, can be explained by increased disintegration of leucocytes; and inasmuch as the leucocytes form in enormous numbers, it is at any rate probable that there is excessive destruction of these cells, or the body would become flooded with them. At a recent meeting of the German Medical Congress at Carlsbad, last year, Professor Minkowski, of Strassburg, described some experiments which he made with the view to determining whether there was any essential chemical difference between polynuclear leucocytes and lymphocytes; and as far as his researches had gone they led him to conclude that there was no essential difference.

The next question that is suggested, is, what is the cause of this disease? Experimentalists have thought of a microbe, and many investigations have been made from this point of view, so far with negative results. At the same Congress at Carlsbad, Professor Löwit, of Innsbrück, announced the discovery of a spore-bearing organism in the blood, a hæmamœba. He maintained that this body could be found not merely in the circulating blood, but also in the organs post mortem; and, still more important, he asserted that by the inoculation of leucocythæmic blood into animals the disease could be produced experimentally. Of course if that point could be established the whole pathology of the disease would undergo a complete revolution. Hitherto it has been generally admitted that leucocythæmia cannot be transferred from one individual to another, although it is believed to occur independently in the lower animals. In the discussion Professor Löwit's statements were naturally severely criticised, and it remains to be seen whether other

observers will succeed in discovering anything of the nature of the hæmamœba. Löwit further asserted that another form of hæmamœba could be found in Hodgkin's disease, both in the child and in the adult. To this disease the Germans give the name of pseudo-leukæmia. If this should prove to be the case, it will help to explain certain cases which lie on the borderland between Hodgkin's disease and leucocythæmia.

It is possible that a chemical poison, not bacterial, may be concerned in the causation of leucocythæmia, for it is found that if peptone and certain drugs be injected into the blood leucocytosis ensues. This leucocytosis is only temporary, and perhaps it is hardly comparable to leucocythæmia; still, the fact is a suggestive one. Drugs have been employed in pneumonia to stimulate leucocytosis, with the idea that the leucocytosis has a salutary influence on the course of the disease.

I shall now briefly describe three cases which we have had lately, which illustrate the transitional cases to which I have alluded.

The first case was a Polish child, Frida Z—, aged fifteen months. It was brought to the hospital on July 2nd, 1898, and died on August 10th in the same year. The patient's parents spoke no English, and we could obtain no history at all. On admission the child was very anæmic and wasted, and numerous purpuric spots were scattered over the face, chest, abdomen, and legs. The ribs were slightly beaded, the fontanelles open, the lower ends of the radii somewhat enlarged. There was swelling and tenderness over the lower half of the left femur and over the left forearm below the elbow. The lower part of the humerus above the elbow was considerably swollen and the skin around ecchymosed. Just near the elbow there was a small wound discharging pus. The child was tender all over, and cried when touched. The gums were not spongy. The abdomen was very much enlarged, and the spleen could be felt two or three inches below the ribs; it was very firm but not tender. The urine presented no abnormal features. On examination of the blood the red corpuscles were 2,000,000 to the cubic millimetre; leucocytes, 72,000, hæmoglobin, 35 per cent. On making film preparations of the blood we found that the leucocytes were represented as follows:—lympho-

cytes, 64 per cent; myelocytes, 20 per cent; polynuclear leucocytes, 16 per cent; and a very few eosinophile cells. There were also nucleated red corpuscles in fair numbers. The child had a more or less constant pyrexia. It was put upon arsenic and antiscorbutic diet, and to a certain extent it improved; there was also a slight improvement in the blood, the number of red corpuscles increasing somewhat, but the leucocytes remained stationary. The improvement, however, was only temporary, and the child began to fail, convulsions ensued, and she died about seven weeks after admission.

The child was admitted for "scurvy rickets," that is to say rickets and scurvy associated. It was true that the child was slightly rickety, but it was noted that there was no sponginess of the gums, and the condition of the blood suggested the conclusion that the child had leucocythæmia. I had the advantage of Dr. Stephen Mackenzie's opinion, who saw the child with me. Dr. Mackenzie doubted the existence of any noteworthy degree of rickets, and was strongly of opinion that it was not simply a case of rickets.

Post-mortem examination.—Costal cartilages moderately enlarged. Lungs collapsed. Mediastinal glands not enlarged. Heart, 1½ oz., contained a few small clots and a small quantity of liquid blood. Spleen, 3 oz., enlarged, and of a dark purple colour on section. Kidneys, 2 oz., pale. Mesenteric glands slightly enlarged, of a dull red colour, and contained a few whitish patches. Alimentary canal normal; no parasites. The brain and its membranes normal. Cranium, ossification marked, no signs of rickets. The left femur showed marked enlargement in its lower half, the bone being expanded over the central cavity, which was occupied by a firm red blood-clot. The swelling of the bone which we attributed to subperiosteal hæmorrhage proved to be due to hæmorrhage into the central cavity of the femur—a very unusual state of things. Microscopical examination of rib shows slight rickety changes only. Sternum normal to naked eye.

The second case, Leonard H—, aged sixteen months, was admitted on January 13th. He was admitted from the out-patient department with rickets and a petechial eruption over the trunk, arms, and legs. The history was that he was the second child in the family, and was born at eight

months. At three months of age he was vaccinated, and was weaned almost directly afterwards. Since that time he had been constipated, very fretful, and had a ravenous appetite. At four months he had a discharge from both ears, and a doctor said he had "inflammation of the membranes of the head." He made a recovery from that illness, but had remained pale and weak ever since. At this time the parents noticed a red rash on the buttocks, which were tender, and the mother said "almost bleeding." About the same time his mouth became sore. Two weeks before admission the child became distinctly ill, dark red spots came out on the neck and face, and he vomited all the food he took. The motions were very offensive. Appetite voracious. The mouth also was said to be sore. On admission the child was very anæmic, and had a slight degree of rickets. Examination of the blood showed 62 per cent. of hæmoglobin, and at first no counting of the corpuscles was carried out. The abdomen was large, the spleen being very much increased in size. The liver was enlarged. There were some slightly enlarged shotty glands in both groins. The child was put upon arsenic, and for a time improved. After about a fortnight's administration of arsenic we found the following state of things:—red corpuscles, 4,720,000; white corpuscles, 70,000. Film preparations of the blood showed the following characters:—lymphocytes, 38 per cent.; polynuclear leucocytes, 43 per cent.; myelocytes, 19 per cent. Therefore the polynuclear leucocytes showed a relative decrease, the lymphocytes a relative increase, and myelocytes were present in fair numbers. The blood contained a large number of nucleated red corpuscles.

The progress of the case may be briefly described. The child frequently had small purpuric spots on the skin. In April the back became œdematous and the pallor increased. The temperature was at first normal, but for the last two weeks it was irregularly remittent, reaching 100° to 102° at times. The patient died of exhaustion. At the post-mortem examination, which was made by Dr. Hadley, the following state of things was found:—Broncho-pneumonia in both lungs; the heart contained soft clots. The spleen, 8 oz., large and firm, surface somewhat rough, no infarcts. Microscopically there was

some increase of fibrous tissue throughout the spleen, and some of the Malpighian bodies were fibroid. Kidneys normal. Liver pale, no iron reaction. From the post-mortem appearances Dr. Hadley was of opinion that this was a case of splenic anæmia; but from the microscopical appearances of the blood it appears to bear closer relation to leucocythæmia than to splenic anæmia. When we use the term splenic anæmia we are dealing with a very uncertain entity. Splenic anæmia, as far as I know, has not been described as occurring in infants.

The last case is that of a boy, M. S.—, aged six, who was admitted on December 24th, 1898, and died on February 2nd, 1899. The parents, Russian Jews, came over to England six months previously. At this time a lump appeared in the child's neck, and increased in size. He appeared quite well then, and till a fortnight ago was running about and doing well. Two weeks before admission the neck and left eyelid swelled, and the scrotum became swollen subsequently. On admission the patient was intensely anæmic, with a peculiar waxy tint of the skin, which was dry and hot. Temperature, 103° to 101°. Large, soft, elastic glands in the neck on the left side about the size of an unshelled walnut. Nutrition was fairly good. There was thought to be less resonance at the apex of the left lung than on the right side. The heart was normal, pulse rapid but small. The abdomen was full, and the spleen was much enlarged. There were no spots. Motions loose and yellow. The urine was acid, and presented no abnormal characters. There were no changes discernible in the fundus oculi. The patient made no improvement at all. He was extremely feeble and semi-comatose, and died a few days after admission.

Fortunately before he died his blood was examined, but it was difficult to get sufficient blood to test the percentage of hæmoglobin. Red corpuscles, 800,000 per cubic millimetre, showing an enormous decrease; white corpuscles, 50,000, showing a considerable increase. The blood was very watery and pale. Film preparations:—Lymphocytes, 43 per cent.; polynuclear cells, 48 per cent.; myelocytes, 9 per cent. Eosinophile cells, very few; nucleated red corpuscles in small numbers. The day before the patient's death he became very drowsy, there was more œdema on

the left side of the face, and we thought there was some paresis on that side, but this was doubtful. The left arm was more flaccid than the right; but he lay upon the left side. The temperature remained remittent and showed the same character throughout the illness, varying from 100° to 103° .

Post-mortem examination (by Dr. Hadley).—A small quantity of fluid in each pleura. Heart, uncontracted and flabby, a small quantity of clot on the right side only. Spleen, enlarged and irregularly nodulated, the nodules having a white appearance and being about the size of a pin's head, scattered through the substance of the spleen. The nodules seemed to be not entirely limited to the Malpighian bodies. Liver, pale but normal. There were enlarged glands on the left side of the neck, extending down to the scapula. There was no enlargement of the mesenteric, axillary, or inguinal glands. On section the enlarged glands were pale, not caseous. In this case, too, there was some doubt as to the diagnosis. Dr. Hadley regarded it as a case of lymphadenoma.

Now the question arises, are these cases of the same nature, or are they not? I think the first and the second are of the same nature. They presented the following features:—anæmia, enlargement of the spleen, marked changes in the blood. These changes in the blood consisted of definite leucocytosis varying from 60,000 to 70,000 leucocytes per cubic millimetre, but the chief increase of leucocytes was represented by lymphocytes. In both cases there were myelocytes and nucleated red corpuscles. The two cases are essentially of the same nature. Are we to regard them as instances of abnormal or atypical leucocythæmia, or are they cases of irregular Hodgkin's disease? I am in favour of the first hypothesis, on account of the condition of the blood, the absence of notable enlargement of glands, and the marked enlargement of the spleen, which is not common in Hodgkin's disease, though it may occur.

It may be suggested that they were simply severe cases of rickets, a disease in which changes in the blood are known to occur. But in answer to this it must be pointed out that the degree of rickets in each case was very slight, and in the first child its very existence had been doubted.

About the third case there is, also, some doubt. This certainly resembles, in many respects, an ordinary sense of lymphadenoma, though the condition of the blood presents some difficulties.

An interesting case, published by Dr. Rolleston and Dr. Latham ('Lancet,' 1898), may be referred to in connection with this point. In an infant aged eighteen months, the subject of rickets and lymphadenoma involving the cervical glands and stomach, the blood showed a leucocytosis of the following character:—lymphocytes, 61.1 per cent.; polynuclear cells, 16.5 per cent.; myelocytes, 20.8 per cent. At the autopsy the spleen weighed 8 oz., and the ribs showed rickety changes and some lymphocytic infiltration of the costochondral junctions and shafts.

The authors regard their case as one of lymphadenoma and rickets, and attribute the blood-condition to the effects of the combined diseases. They point out that cases of Hodgkin's disease may develop a condition of blood closely resembling leucocythæmia towards the close, a point that I have been able to verify myself in two cases. As they further remark, our knowledge of the varying conditions of the blood in young children is still very imperfect. In a recent discussion at the Pathological Society (May 1st, 1900) Professor Wright stated that in the blood of children the polynuclear cells rarely exceeded 40 per cent. of the total leucocytes, whereas in healthy adults these cells are present in proportions of 70 to 75 per cent.

It is evident that a further study of the blood of infants and young children is needed to enable us to classify the different forms of anæmia occurring in early life.

Beginning Signs of Diabetes.—M. E. Dufourt says that the first signs of diabetes may manifest themselves in a great variety of ways. The nervous system, the respiratory tract, or the genital organs may be the first to give evidence of the disease. In 350 cases, 219 complained solely of polydipsia as a first sign of diabetes. Lassitude, neuralgia, insomnia, neurasthenia, and syncope were, in the order named, the principal complaints. Diabetes, then, may show symptoms in a number of different ways before attention is directed to it.

N. Y. Med. Journ., June 23rd, 1900.

The Treatment of Pelvic Neuralgia.—

Pelvic pain may arise from metritis, salpingo-ovaritis, cellulitis, or pelvic peritonitis. It is met with in women after the menopause, and is often associated with lumbo-sacral neuralgia.

An examination of such cases often fails to reveal any abnormality in the directions we have already named; and in these instances it should not be forgotten that the pains may be rheumatic in character, and that one of the salicylates may be advantageous.

One method of treating this condition would be the administration of 5 to 15 grains of salicyn three times a day, the drug being placed in cachets or capsules, taken after meals, and followed by a glass of water. In other instances other salicylates may be tried when salicyn disagrees with the stomach—as, for example, salol, or the new preparation, aspirin.—*Journ. de Méd. de Paris*, Feb. 11th, 1900. *Therapeutic Gazette*.

'Chambers' Journal' recently published two articles which, from their importance to the every-day man and woman, deserve special notice. "The decline of oatmeal porridge" and "The advent of shredded wheat biscuit," two ordinary enough titles, convey in a limited sense only the real importance of the subject. It has very truly been written, "Man is built out of the food he eats;" and were this evident truism but more generally acted upon, it would be very greatly to the advantage of those who put "precept" into practice. On all sides—medical as well as lay, it is an accepted fact that we eat either injudiciously or too much; and the great importance underlying this fact is gradually gaining ground. The physiology of foods, the study of bacteriology in so far as it bears upon this important, ever-present question to the average man and woman, of "what shall I eat?" is fast reaching a stage where a more wholesome and a more sustaining diet becomes imperatively necessary. Science compels us now-a-days to cast away many old and long-established errors, and the errors of diet do not escape the general effects of a reasonable and scientific examination into the long list of either harmful or unsatisfactory building materials daily consumed under the head of food. From time immemorial it has been conceded that a *cereal food* is one of the great desiderata to be aimed at, and wheat has ever been with the great bulk of the human family *the* cereal. The many great improvements in this direction of late years have resulted in the almost innumerable varieties of breads obtainable, but they are all more or less faulty in that they do not

contain a true proportion of those elements which are the real food bases. The accompanying analysis of wheat and shredded wheat biscuit speaks for itself on the all-important subject of purity, and gives us the assurance also that in shredded wheat biscuit we get a true building material, not only a common-sense food, but a pure, nourishing, and satisfying diet.

	Raw Wheat.	Shredded Wheat.
Moisture ...	10.60	10.57
Ether extract ...	1.75	1.03
Fibre ...	2.40	2.58
Ash ...	1.75	2.65
Proteins ...	12.25	12.06
Soluble starch and other carbo-hydrates ...	71.25	71.11

The variation, a very slight one, is caused chiefly by the great heat in the process of manufacture. It is safe to assert that there is to-day no food obtainable which is more carefully prepared; it is not only clean, but absolutely pure and absolutely void of adulteration. Containing, as it does in an easily assimilated form, the soluble phosphates, it is at once fitted to become a universal and an every-day diet. Sick and well, robust or invalid, young or old, can by using it reap not only "length of days," but more vigorous enjoyment of life, and can more easily and efficiently do their work. In America it has been exhaustively tried by physicians; it is in constant use in the great hospitals and nursing institutions, and by specialists in dietetics. In this country as in that, where the rush and hustle and stress of life are every day claiming their innumerable victims, it has become a daily fact at almost every table. To the well and strong it is hard to appeal so long as they remain well, but let even slight indisposition seize them, and their almost unanimous question is "What have I taken that has upset me?" Most medical men unhesitatingly say that many cases of "feeling out of sorts," "being a bit run down," etc., are distinctly traceable to injudicious and badly nourishing food. As an aid in the treatment of dyspepsia in its many forms, in anemia and chlorosis, in rheumatism, gout, and the uric acid diathesis, in the neurasthenia of women, its use is indicated, and has given most excellent results. In strumous affections of children and as a food for the young it is admirable. In conclusion, it is only necessary to add that it is cheap, and everywhere obtainable. It can be quickly and easily made into a great number of appetising dishes, and it undoubtedly is what it claims to be, viz. a palatable, nutritious, and a common-sense food.

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A CLINICAL LECTURE ON HEAD INJURIES.

Delivered at the Westminster Hospital
By CHARLES STONHAM, F.R.C.S.,
Surgeon to the Hospital.

GENTLEMEN, —The case on which I propose to lecture to you to-day is that of a man aged 68, who unfortunately died last night. He was found at the bottom of six stairs, down which he had apparently fallen. He had been lying there for some time, as his son found him at four o'clock the morning. The patient was capable of being roused, and the son helped him into his bedroom, and although he appeared dazed, yet he said that he felt all right, and refused to come to the hospital. I may say that this patient had been a pretty heavy drinker, and for three weeks before this accident he had apparently been on a drinking bout, and therefore it is probable that he fell downstairs while drunk. He was put to bed, and remained there till the evening of the 7th, when he voluntarily got up for three hours. At that time he appeared quite rational. On the following day he stayed in bed all day, but appeared quite sensible until the evening, when he rambled a little in his speech. He slept well at night, and early on the morning of the 9th he was again rambling. He still declined to come to the hospital, but was brought forcibly.

It appears that when this man was found no blood was noticed to be coming from his ears or nose; nor was there any discharge noticed from his ears up to the time of bringing him to the hospital. It was noticed that when he was in bed during the three days he was at home he passed his water under him, but that he had control over his motions.

On admission the condition was as follows:—His pupils were equal, and in a mid-state between contraction and dilatation; they reacted to light, though slowly. Inside the left ear there was some dried blood, but no fresh bleeding occurred after

his admission, nor was there any escape of cerebro-spinal fluid. His breathing and pulse were normal; he was semi-unconscious, but could occasionally be roused and made to answer questions. He passed both *fæces* and urine unconsciously. As a matter of fact he had paralysis of his bladder, the urine dribbling away from the over-distended organ. There was no paralysis or rigidity on either side; he occasionally puffed his cheeks out on expiration. There was a healed scalp wound over the left parietal region, on which was some coagulated blood. When he was admitted, on the 9th, his temperature was 100° , and at times varied from the normal to 100° only; on the 11th it ran to 103° . It then varied between this point and 101° till he died last night; the temperature was then 103.6° .

The diagnosis I made in this case was that the man had a fracture of the base of his skull, with some slight laceration of the base of the brain, and consequent hæmorrhage. (This was confirmed by autopsy). The gravity of this case, and indeed of any head injury, depends upon other factors than the immediate damage that may be done to the bones, such, for example, as fracture of the skull; it is not the fracture itself which is the grave part of the mischief, it is the concomitant injury to the brain, to the nerves, or possibly to blood-vessels. Such concomitant injury may be slight, from which the patient may recover, but possibly leaving, however, some more or less remote after-effects, which may be of exceeding gravity; or the immediate injury to the cranial contents may be so serious that the patient dies either at once, or at any rate within a few hours or days, as in the case which I have described to you.

It is impossible, of course, in a clinical lecture to go over the whole subject of injuries to the head, and it therefore seems to me that the best thing I can do in this and my next lecture is to direct your attention to the more common conditions which may be met with in association with fracture of the skull, and to point out how these can usually be diagnosed, and what is the proper treatment.

But at the outset you must clearly understand that, although in a description of head injuries one follows a more or less hard and fast line, taking really an uncomplicated case as the test, in actual practice these cases are sometimes ex-

ceedingly difficult to diagnose, for the reason that there is an association of damage—such as concussion complicated by symptoms of cerebral laceration, and supervening upon these, perhaps, symptoms of compression. Further, in some cases additional symptoms will accrue from secondary inflammatory results in the brain, and possibly septic meningitis or some septic mischief in the bones.

Fracture of the skull may affect either the vertex, the base, or both, and the question as to whether the vertex or the base is affected depends in great measure upon how the injury is inflicted. If a blow is delivered on the top of the head, and is not very severe, it will break the skull at the point struck; but if the injury is severe the fracture will pretty surely extend also into the base, and the fossa which is broken at the base will correspond to that segment of the skull which is broken at the vertex. The skull is divided into three zones, corresponding to the anterior, middle, and posterior fossæ respectively. A blow in the anterior part of the vertex will, if it extends to the base, break the anterior fossa; if the blow be in the middle part of the vertex the middle fossa will suffer, and so on. But at the same time you must bear in mind that if the force be very great all three fossæ may suffer, even with a fracture which does not implicate the vertex to any extent, or independently of such. When a man falls on to his head on to a resisting substance, such as the ground, the probability is that it will break his vertex, and perhaps also the base. If he falls some distance on to a yielding substance he is more likely to break the base than the vertex. The reason of this is, that in the case of falling upon a hard substance, it is the hard substance which is the fracturing force; but if the head strikes a soft yielding substance, such as sand, the patient's body is the fracturing force. Fractures of the vertex are, as a rule, much less serious injuries than are fractures of the base. A very important distinction between the two is that whereas fractures of the vertex are frequently simple, fractures of the base are more commonly compound; thus they may be either compound through the nose or through the body of the sphenoid and into the pharynx, or, most commonly of all, they are compound through the ear. I need hardly point out that this renders the

subsequent dangers greater, and makes the prognosis far more grave, not only because injuries of the base require greater force to inflict them, but being compound in the situations indicated you cannot secure asepsis without great difficulty, and hence septic inflammation may occur.

Fractures of the vertex are usually linear. Not uncommonly there are several lines of fracture radiating from the point struck, giving rise to stellate fracture. If the line of fracture reaches a suture, one of three things may happen. It may either stop at the suture, go right across it as if it did not exist, or it may run down the suture for a varying distance, and come out on the other side. Which of these conditions occurs depends on the rapidity with which the fissure travels, this being dependent on the amount of force applied. Fissured fractures, if simple and uncomplicated, are very difficult to diagnose; in fact, you can only suspect them by the concomitant signs of concussion, and possibly laceration of the brain. A fissured fracture which is produced by a very localised force, such as a blow from a hammer, is likely to be very severe at the point struck, and parts of the bone will probably be comminuted and depressed. Depressed fractures may be either simple or compound. As a rule in adults a depressed fracture is compound.

Occasionally depressed fracture is simple, and such is much more likely to be the case in children, because in them the skull is much more easily indented; it is comparatively elastic, and the soft structures may escape injury. The skull of a child may be indented by only a slight blow.

The signs of depressed fracture will be, first, those which are due to the mere fact of want of regularity of the skull; secondly, those which may be dependent upon associated laceration of the brain; and thirdly, those directly due to the cerebral compression.

Now as to the general signs of fractures of the skull. As I have said, in simple fissures of the vertex the diagnosis is purely inferential; you cannot make quite sure of it. But in the case of compound fissured fracture you have only to examine the bone after cleaning the wound, and you will see a fissure running across the bottom of the wound as a thin red line, a little blood oozing up between the edges of the fissure, which can be easily felt as a slight irregularity in the bone. In

the case of depressed fracture the diagnosis is perfectly easy, whether the injury be simple or compound. You will find definite irregularity of the skull cap, and possibly symptoms of compression, which symptoms may not, however, come on for some time, or even at all. The symptoms indicative of fracture of the base are bleeding and the escape of cerebro-spinal fluid. The bleeding, you must bear in mind, is characterised by its amount, by its duration, and by the time of its appearance; it is not the fact of bleeding, but the manner of the bleeding which is diagnostic. If a man receives a blow upon the head he may bleed from his nose or from his ear, or may vomit blood without having a fracture of his base, because he may have sustained injury to some of the soft structures, and consequently bleed from them. But the points about such bleeding are that it comes on immediately after the injury, is rarely very profuse, and usually ceases very quickly; and when it has once ceased it does not recur. The bleeding in the case of fracture does not usually make its appearance until some little time afterwards. It is always more or less profuse, and persists for some time; thus it may keep on dribbling from the ear for many hours.

The situation of the hæmorrhage is important in locating the seat of damage to the skull. Hæmorrhage from the ear indicates a fracture of the middle fossa of the base, but you must remember that it may be due to rupture of the membrani tympani, or some associated damage to the ear without fracture. If the blood passes into the pharynx by the Eustachian tube it may escape from the nose, or, being swallowed, may be subsequently vomited.

Hæmorrhage into the orbit is a sign of fracture of the anterior fossa of the base of the skull, but it is also commonly met with in head injuries as an ordinary black eye or subconjunctival hæmorrhage. But the distinction between simple subconjunctival hæmorrhage and that due to fracture of the roof of the orbit is that the hæmorrhage occurring as the result of fracture does not make its appearance until some little time afterwards, and further, that the blood comes from behind forwards; and therefore, if you lift up the lid you will see no ring of white sclerotic behind the blood, but you may see a ring of white sclerotic in front of it. Now if the bleeding is dependent upon rupture of the

subconjunctival vessels you will very probably see a band of white sclerotic behind, showing quite clearly that the bleeding did not come from behind forwards from the skull. At the same time it must be remembered that a large subconjunctival hæmorrhage will extend far back into the orbit.

With regard to the escape of cerebro-spinal fluid, it does so by exactly the same routes as does the blood—that is to say either from the nose or from the ear. Sometimes it escapes through fractures of the vertex. Such escape implies that the subarachnoid space has been opened, and that there is a direct communication between the external seat of escape and the subarachnoid space; consequently the injury is generally a very severe one, and as a general rule patients who are losing cerebro-spinal fluid die. Cerebro-spinal fluid does not usually begin to escape until twenty-four to thirty-six hours after the accident, but when it does start it may continue for a week or ten days, and pints of it may come away. It is a perfectly clear watery fluid; sometimes at first it is tinged with blood, but afterwards it comes away quite clear and watery. It is composed of water and sodium chloride, with a copper-reducing substance.

Such are the signs which you would meet with in cases of fracture of the skull, *qua* fracture; but important signs are also induced by complications.

Now the complications of any fracture of the skull—that is to say the immediate complications—are concussion, laceration, and compression of the brain. The later results may be due to inflammation of the brain or its membranes, some septic trouble occurring at the seat of fracture, and certain mental conditions which may follow on any grave cerebral injury—such, for instance, as epilepsy, mania, irritability of temper, and so on.

Let us first take concussion of the brain. Concussion of the brain is a term given to an assemblage of symptoms which ensue on any severe injury to the head, sometimes without any obvious pathological cause, sometimes associated with a cerebral laceration, and varying very markedly in degree. You may see all grades of severity of concussion of the brain, from a mere temporary stunning, such as most of us have experienced at some time or other in our lives, up to a state of profound unconsciousness which proves fatal in a short time. There has been a considerable amount

of discussion as to what concussion of the brain really is. It has been stated that it is dependent upon vibration waves in the brain occasioning molecular paralysis for the time being. It seems to me that the most likely theory of concussion is that it is dependent upon an alteration in the tension of the cerebro-spinal fluid. The cerebro-spinal fluid is driven by the force of the blow from the lateral into the third ventricle. It, however, flows into the third ventricle at a greater rate than it can escape from it, and there is, in consequence of this, an increase in the intra-ventricular tension. Such increased tension in the ventricle stimulates the restiform bodies and produces cerebral anæmia. It is a noticeable fact that in a patient who dies of concussion you always find more or less cerebral laceration, accompanied by petechial hæmorrhages, and sometimes profuse hæmorrhages under the pia arachnoid; and it is assumed by some that in all cases of concussion there is a certain amount of hæmorrhage and laceration, the only difference being one of degree. According to Duret, the existence of pia matral extravasation in concussion is explained by the increase in the ventricular tension. He points out that if the pressure of the cerebro-spinal fluid is removed from the cortical vessels they are, in a manner, placed in vacuo, and are consequently no longer able to withstand the normal pressure of the blood-stream, and accordingly rupture. In accordance with this view he insists that, instead of the symptoms of concussion being due to the hæmorrhages, the hæmorrhages are due to the onset of the concussion.

Next as to the symptoms. The main diagnostic points of cerebral concussion are—(1) that the concussion comes on immediately on receipt of the injury, and (2) if the patient survives he gradually gets better. That is in marked contrast to what usually happens in cases of compression, the symptoms of which come on some little time after the injury; and unless surgery steps in and removes the compressing agent, the symptoms gradually deepen until the patient dies.

The symptoms of concussion may be conveniently divided into two stages: first, there is the stage of collapse; secondly, the stage of reaction. The interval there is between these two stages, or how soon the stage of reaction supervenes on the collapse, varies with circumstances. The more severe the concussion, the longer will the collapse

last and the more tedious will be the convalescence. In a mild case the patient feels stunned, dizzy, and confused, but pulls himself together again after a time, without, perhaps, requiring active treatment. During the stage of collapse in a severe case the patient lies as if he were dead; he is more or less unconscious, and may be profoundly so. The skin is pale, and the pallor of the face is very noticeable; the respirations are shallow, in fact, so shallow that you may be in doubt whether the man is breathing or not. The pulse is slowed, extremely weak, and may be even imperceptible. The pupils are usually widely dilated, but that is not always the case; occasionally they are contracted. The sphincters are relaxed, and the patient's temperature is depressed; it may be depressed as low as 95° F. The patient may remain in this condition for minutes or hours; occasionally he remains like it for a day or two. As soon as the stage of reaction comes on the patient begins to vomit. Vomiting is not only a sign of recovery, but it whips up the heart. The first thing you will notice when reaction comes on is that the patient begins to move or shift his position; the muscular feebleness is passing off. His temperature begins to rise, and may reach in the course of a few hours 101° or 102° F., sometimes even higher. The respirations also become more marked, the pulse stronger, and the skin loses its pallid tint and resumes a more healthy appearance. You will, perhaps, also observe that the patient begins to take notice of what is said to him, but takes a long time to collect his thoughts; and, although he may answer a question, he will contract his brows and look puzzled before doing so. But if you try to follow that question up by another, you will find he is totally incapable of carrying on any sustained train of thought, and I need hardly point out that it is most inadvisable that you should attempt to make him do so. The stage of reaction is usually uninterrupted, and the patient gradually convalesces.

Following the stage of reaction— it may be more or less immediately or much more remotely, perhaps even months or years elapsing—there are certain sequelæ which sometimes occur, and must not be lost sight of. Of these I may mention a certain degree of mental inadequacy, epilepsy, mania, and perhaps a great alteration in the man's habits; he may lose his business aptitude, interest

in his surroundings and relations, and perhaps from being a good tempered man he may now be irritable and morose, or the reverse may happen. Sometimes these patients become absolutely intolerant of alcohol, as is popularly known, and I need hardly say is made considerable use of in the law courts when a man is in trouble for being drunk.

The treatment of concussion is simple enough. You must encourage the action of the heart, and the best thing is to put the patient into bed between warm blankets, raise his extremities, and let the head lie flat, and then apply friction over the cardiac area. If he is very bad you should give him a hypodermic injection of ether. I may say at once that you ought never in a case of head injury to give stimulants unless you are driven into a corner. It may be that the patient is so bad that if you do not stimulate him his heart will fail and he will die. And in such a case you must risk something, and give stimulants proportionately to the need of the patient. The reason for not giving stimulants in cases of head injury is that you do not know to what extent the vessels of the cerebral cortex have been damaged, and if you increase the strength of the heart-beat by stimulants you may increase bleeding from those vessels. Further, it appears that giving stimulants in cases of head injury is very likely to encourage cedema of the brain, and inflammation of the brain and membranes as a subsequent condition. Therefore the rule is, do not stimulate such patients.

Food must be given in such quantities and by such means as are possible. It is very rarely that a patient who is badly concussed can take food by the mouth, and you must therefore feed him by nutrient enemata. I need hardly say that the patient must be kept absolutely quiet, usually in a darkened room, and with ice-bags applied to his head. During the stage of reaction no active treatment is required; all you have to do when the patient is recovering himself is to keep him quiet, and free from mental disturbance.

It is a very important thing indeed, when a man has been concussed, to let him have absolute and prolonged rest after the injury. It does not matter whether he has been severely or only slightly concussed. The surest way to avoid after-effects is to give him a chance of perfect recovery. In hospital

practice we cannot, as a rule, ensure this, for the patient has to go back to earn his living, and a matter of days may decide whether he will keep his work or lose it. But in private practice you should insist upon it, and if the patient has been badly concussed your best course is to advise six months' absolute rest. During this time he should abstain from all business or worries of any kind; he should not bother himself about anything whatever. The best plan is for him to go down to a little country place, and avoid all excitement. Many of these patients or their friends will ask you to let them go to some well-known health resort, but this should be discountenanced, for such places are not calculated to afford the mental rest which is so needful. I once heard a London surgeon say that the best thing for the patient is to go down into Kent, lie on his back, and watch the apples grow.

Next a few words as to laceration of the brain. Laceration of the brain usually occurs on the side opposite to that struck, and is due to the impact of the brain against the bone. It may also be met with at the point struck if the bone is splintered and depressed. The extent of the laceration varies within the widest limits. Laceration from direct injury is the most extensive, and sometimes masses of brain escape through the wound. But such cases I think hardly come under our consideration. The cases I wish to direct your attention to are less severe, and are caused by indirect violence. Lacerations of this nature are more commonly met with at the base than on the surface, and on the anterior rather than on the posterior surface. The reason of this is that violence is usually applied to the vertex of the skull, and therefore the base of the brain is the part most usually damaged. The reason that the posterior parts of the brain escape is because they are slung upon the tentorium cerebelli, and even the cerebellum itself lies in quite a smooth fossa, whereas all the rest of the under surfaces of the brain are unsupported by an elastic membrane like the tentorium, and lie on a more or less irregular and uneven base.

Laceration is accompanied by more or less hæmorrhage. The hæmorrhage may be confined by the pia-arachnoid, and may occur into the substance of the brain; in other cases it bursts the pia-arachnoid, and becomes diffused underneath

the dura mater. Exactly what happens depends on the amount of hæmorrhage, and on the severity of the laceration.

The symptoms of laceration are associated with those of concussion, and in the very early stages of the condition you will not diagnose it. You may assume it to have occurred from what you know of the nature of the injury. What you will find is that a patient is concussed, and in a few hours, or it may be in two or three days, the diagnostic symptoms of laceration begin to come on. These are either tonic contractions of certain muscles, or groups of muscles, or spasm of certain groups of muscles. As far as I saw in the man who has recently died, there was no rigidity of his muscles. I thought at one time that his legs were somewhat rigid, but on going into the matter more fully I could not satisfy myself that it was so. But patients with cerebral laceration often fall into a peculiar condition known as "cerebral irritation." I had a very good instance of that in here just before I went away in the summer. In that condition of "cerebral irritation" you will generally find the patient lies curled up on one side of the body. He is generally flexed, and his spine is strongly arched. His fingers are clenched in his hand, his wrist is doubled on the forearm, and the forearm doubled on the arm. He lies curled up in a heap. The eyes are tightly closed, and the patient will absolutely resist any effort you make to look at his pupils. But if you can look at them you will find generally that they are dilated equally, but occasionally unequally. They react to light. As a rule micturition and defæcation are involuntary, the patient passing everything under him in bed. He is semi-conscious; you can rouse him, and you can get him to pay attention to you. But the main characteristic of his mental condition is a frightful mental irritability. The man is as bad tempered as he can be. He is so extremely bad tempered that if you try to feed him, or if you try to turn him over to look at him, he exhibits great irritation; in fact, it looks as if the man had become bad tempered and surly rather than anything else. He may remain in a condition like that for two weeks, three weeks, or even a month. One man I remember was like it for three weeks. Then he gradually begins to get better, and the first thing you notice is that instead of being coiled up he begins to uncoil himself, and shifts his

position more as the affection disappears. But you will find that although the man may completely recover eventually, it is by no means uncommon to find that his irritability of temper gives place to an abnormal stupidity. It seems as if some lesion had occurred in his psychic centres, by which they were at first made more irritable and impressionable, and that subsequently they had very much deteriorated, until the man becomes almost a drivelling idiot. That is not always the case. The man we had here two or three days ago was of weak intellect normally they tell me.

The treatment of cerebral laceration is virtually that of concussion. If the laceration is severe the prognosis is fatal, and your patient will die within two or three days. If the laceration was associated with a large amount of hæmorrhage, so as to produce compression of the brain, it might be a justifiable course to trephine and expose the seat of the laceration to see if there was any definite vessel which could be tied. As a matter of actual fact you will find that that is all very well in theory, but in practice it is an exceedingly difficult thing to do. And even if it were not difficult, it is almost hopeless. I have tried it on two occasions, and what I found was that although I could tie perhaps one vessel or two vessels, yet there was hæmorrhage going on from minute vessels for which I could do nothing. My own feeling is that if I diagnose laceration without depression I should leave the patient alone; I do not think you can gain anything by tying vessels in such a case.

The subject of compression and middle meningeal hæmorrhage I must leave until another lecture.

Veratrum in Palpitation of the Heart.—

The 'Medicinische Woch.' for April 23rd gives this formula :

R Sodium bromide ... 1½ drachms.
Tincture of Veratrum viride 50 drops.
Syrup of orange peel ... 12½ drachms.
Distilled water, enough to
make ... 4½ ounces.
M. S. : A tablespoonful twice a day.

New York Med. Journ., June 28th.

TWO CLINICAL LECTURES

ON THE

CONSTRUCTION

AND

ADJUSTMENT OF TRUSSES.

Delivered at the West London Post-Graduate College,
May 21st and 24th, 1900.

By **W. McADAM ECCLES, M.S., F.R.C.S.**,

Assistant Surgeon, West London Hospital, and City of
London Truss Society; Examiner in Anatomy to
the Society of Apothecaries, etc.

LECTURE II.

The Construction and Adjustment of Trusses for Irreducible Inguinal Hernia, for Femoral Hernia, for Inguinal together with Femoral Hernia, and for Umbilical Hernia.

GENTLEMEN,—This afternoon I desire to demonstrate to you further upon the construction and adjustment of trusses.

The Construction and Adjustment of Trusses for Irreducible Inguinal Hernia.

In dealing with irreducible inguinal hernia in which operation is contra-indicated, it becomes necessary to treat the case from a palliative point of view. For an incomplete inguinal hernia the adjustment of a hollow pad attached to an ordinary inguinal truss may suffice. In cases of irreducible scrotal hernia there may be employed either a hinged-cup truss or a bag-truss. The



Fig. 1.—A left hinged-cup truss.

former not only supports the contents of the sac, but also tends to bring about their reduction, at the same time preventing further descent. The bag-

truss, on the other hand, merely acts as a support of the contents of the sac. The hinged-cup truss is fashioned on the model of the forked-tongue, but the tail portion is replaced by a triangular open cup of metal covered with chamois leather. This frame is attached to the pad by a hinge, which allows of the cup being bent upwards

ward pressure of the cup upon the contents of the sac.

The Construction and Adjustment of Femoral Trusses.

The construction of an ordinary femoral truss differs but little from that of an ordinary inguinal,



Fig. 2.—A left hinged-cup truss adjusted.

and backwards, but not in the reverse direction. Its adjustment you will see in this patient (Fig. 2). The two straps prolonged from the lower part of the cup are fixed to movable buckles on the spring of the truss, and the further forward these buckles are placed the greater is the upward and back-

ward pressure of the cup upon the contents of the sac. This portion of the truss is smaller, and extends a little lower than in an inguinal; moreover the spring from the shoulder to the pad has a somewhat greater obliquity. The measurement for a femoral truss must be accurate, for if the truss is too large, the pad is

certain to impinge against the spine of the os



Fig. 3.—A single femoral truss.

pubis, and if too short it will not act efficiently. To obtain the size, the tape-measure should be applied in precisely the same manner as it was for an inguinal protrusion. The application is as for an inguinal truss, but the pad will of itself fall into position over the femoral ring and canal. The under-strap should be placed a little further forward than in an inguinal truss, that is, at, or a little in front of, the shoulder of the truss.

In cases where the femoral protrusion is of somewhat larger dimensions than the typical small rounded hernial swelling, a thigh-belt should be added to the truss, this being attached to a downward prolongation of the soft material of the pad.



Fig. 4.—A right ordinary femoral truss adjusted.

The object of the thigh-belt is to prevent the



Fig. 5.—A femoral truss with a thigh-belt.

extended pad from being lifted out of position

whenever the thigh is flexed. It is of the greatest importance that the angle at which this thigh-belt is attached to the prolongation of the pad should be such as to cause no rubbing in the fork between the thigh and the labium or scrotum. The measurement for the thigh-belt is taken by passing the tape horizontally round the thigh as high up as the fork between it and the scrotum or labium will allow.

The adjustment of the femoral truss with the thigh-belt is a matter of some nicety.

The portion of the pad which contains the metal should lie in precisely the same position as it does in an ordinary femoral pad, the prolongation from it occupying the hollow of



Fig. 6.—A femoral truss with a thigh-belt adjusted.

Scarpa's triangle. The cross-strap having been fastened, the thigh-belt is lifted and brought down so as to lie accurately in the space between the labium or scrotum and the thigh. It is then carried backwards till it comes to encircle the outer side of the thigh, where its two straps are fastened to the two buckles fixed to the outer part of the prolongation of the pad. Finally the under-strap is adjusted in the usual manner, its function being, as before, to draw the spring of the truss down from off the crest of the ilium, and particularly from the anterior superior spine.

The adjustment of this truss is well shown in the accompanying photograph (Fig. 6).

In other cases where the femoral protrusion mounts upwards in front of Poupart's ligament, the addition of an inguinal fulness above the site of the ordinary pad becomes necessary. This consists of a triangular piece of soft material, which from its position above the femoral pad comes to lie over the inguinal canal and the superficial abdominal ring. It is well to remember that the distance between the inguinal canal and the commencement of the femoral canal is in reality measured by the breadth of Poupart's ligament, and thus this addition will protect the weak region. The inguinal fulness is prolonged into a strap which should fasten to a buckle fixed at the shoulder of the opposite side (see Fig. 7). The addition of an inguinal fulness necessitates the addition of a thigh-belt, but a thigh-belt may

truss with an inguinal fulness is similar to that of a femoral truss with merely a thigh-belt.



Fig. 8.—A femoral truss with a hollow pad.

In cases of small irreducible femoral herniæ in which operation is contra-indicated from any cause, the adjustment of a femoral truss with a hollow pad is desirable. This may, by its pressure, ere long bring about reduction of the contents of the sac.

The Construction and Adjustment of Trusses for Inguinal and Femoral Hernia occurring together in the same patient.

When a patient appears with incomplete inguinal hernia on one side and a small femoral hernia on the opposite side, it is best to order a truss having an ordinary inguinal pad together



Fig. 9.—A truss with an ordinary femoral pad on right, together with an ordinary inguinal pad on left.

with an ordinary femoral pad. Usually it is advisable to make the spring of such a truss in two halves, sliding on one another, and adjustable by means of a buckle placed at the back of the truss. By this means the truss can be applied nicely, so that there is no chance of pressure by the femoral pad upon the pubic spine.

The measurement to be taken for this form of truss is like that for an ordinary double truss, an inch being as a rule added, so as to ensure



Fig. 7.—A truss with an ordinary inguinal pad on the right, and a femoral pad with a thigh-belt and an inguinal fulness on the left.

frequently be used without an accompanying inguinal fulness. The adjustment of a femoral

comfort, seeing that the truss will not stretch at any part.

The accompanying photograph (Fig. 10) indicates very clearly the position of the two pads when the truss has been adjusted, and it will be seen that the exact spot of their application is not so very different as to its level, proving defi-

patient presents himself with a scrotal hernia on the left side and an ordinary small femoral protrusion on the right, what is the proper construction of the truss that is suitable for his case?

As a rule there should be a forked-tongue pad on the side on which the inguinal hernia is, and an ordinary femoral pad on the opposite side



Fig. 10.—A truss having an ordinary femoral pad on the right and an ordinary inguinal pad on the left adjusted.

nately the fact that the relation of the inguinal canal to the femoral ring is a closer one than is generally supposed.

The combination of the more elaborate forms of inguinal with the more complicated varieties of femoral pads is a matter that has not infrequently to be carried out. Supposing that a

(Fig. 11). In order, however, that there should be no tendency for the femoral pad to be dragged out of position, it is requisite that the forked-tongue should fasten to a fixed buckle at the shoulder of the opposite side. The usual measurement for a *double* truss is taken.

The application is that of a forked-tongue to-

gether with a femoral pad, and is well seen in



Fig. 11.—A left forked-tongue together with a right
ordinary femoral pad.

the adjoining photograph (Fig. 12).

Where the femoral protrusion is also of a size or form that requires the adjustment of a larger pad than the ordinary one, it is necessary to add a thigh-belt, and even an inguinal fulness. I may show you some trusses which illustrate the construction of these forms of instruments (see Figs. 13 and 14).

Nothing is perhaps more difficult to satisfactorily control than an inguinal hernia reaching the scrotum together with a femoral on the same side. An inguinal fulness does not by any means always successfully retain within the abdomen the contents of the inguinal sac. In some of these instances it may be well to employ a forked-tongue



Fig. 12.—A truss with an ordinary femoral pad on the left and a forked-tongue on the right, adjusted.

truss to which has been added a thigh-belt arising from well up at the shoulder of the truss.



Fig. 13.—A right femoral with thigh-belt and a left forked-tongue truss.



Fig. 14.—A truss with a right femoral pad, with a thigh-belt and inguinal fulness, together with a left forked-tongue.

The Construction and Adjustment of Umbilical Trusses.

In children the best instrument for controlling an umbilical hernia is a properly constructed umbilical truss (Fig. 15). Belt, strappings, and other



Fig. 15.—An umbilical truss for an infant.

appliances are unsatisfactory. The construction of this form of truss for a child is identical with

that of an ordinary umbilical truss for an adult. The measurement to be taken is that horizontally round the abdomen at the level of the umbilicus. In applying the truss the centre of the pad should correspond with the middle of the hernial protrusion. The construction of an ordinary umbilical truss for an adult (Fig. 16) is that



Fig. 16.—An umbilical truss for an adult.

of a large circular pad, slightly concave, springing from which is a band of steel reaching three-quarters of the distance around the abdomen, from the end of which is prolonged a leather strap to fasten to the button on the front of the metal pad. The measurement for an umbilical truss should be taken round the abdomen, across the hernia, with a tape applied somewhat tightly. The adjustment of this form of truss is a matter of some importance. The centre of the pad should not correspond with the centre of the hernia, but rather the upper border of the pad should lie in position with the upper border of the protrusion (Fig. 17). In this way an upward and backward support is given in the upright position, to the usually pendulous abdomen.

In cases of small umbilical herniæ in thin subjects, a Salmon and Ody umbilical truss may be satisfactory. This consists of a semi-circular spring, attached to the ends of which are two pads of unequal size, one, the smaller, to rest on the spine behind, and the other, the larger, on the hernia in front. The two are also connected round the opposite side of the abdomen by a flexible strap, fastening to two studs on the face of the anterior pad.

Where the umbilical protrusion is irreducible and operation is contra-indicated, it may be necessary to apply what is known as a rim-plate umbilical truss. In this the pad consists of a round or oval rim of metal, the open space of which is covered in with chamois leather, so as to form



Fig. 17.—An umbilical truss adjusted.

a shallow bag. The edge of the rim must be accurately adjusted so that it extends somewhat beyond the limits of the circumference of the treatment of umbilical hernia unless supplemented by an ordinary umbilical truss worn external to it.



Fig. 18.—A Salmon and Ody umbilical truss.

protrusion. The cross-strap is to be divided into three, in order to give greater steadiness to the truss. An abdominal belt is of little use in the



Fig. 19.—A rim-plate umbilical truss.

(The illustrations in this Lecture are taken from the author's work on 'Hernia'.)

LOCAL ANÆSTHESIA.

Technique in Schleich's method.

Dr. DUDLEY BUXTON.

"THE solution and syringe having been carefully sterilised and the skin cleansed, the needle is inserted *endermically*, and some solution injected. The wheal resulting is again injected at its periphery, and this is injected in like manner until a linear zone of skin is rendered œdematous by the fluid introduced. Subsequent injections are made hypodermically, and when the deeper structures are to be operated upon these are also injected. When the structures are cut, much of the œdema fluid flows away, but the tissues remain analgesic for some twenty minutes. Repeated injections are made as the operation is in progress. Large quantities of the solution may be employed.

"Nervous patients are not good subjects, as they complain, are restless, and sometimes get faint and vomit. The most extreme care must be taken to insure asepsis in the tissues and instruments. Inflammatory areas are with difficulty rendered painless, and these as well as those invaded by morbid growths are liable to the danger of having the *materies morbi* injected into healthy areas during the infiltration. All loose tissues such as those about the tongue must on no account be infiltrated, as death due to œdema glottidis from escape of the injected solution has resulted. It is practically impossible to render analgesic the hollow viscera, e.g. stomach, by this method; I have seen great shock occur when the viscera were handled during an operation under the infiltration method, even though the viscus had been carefully irrigated with the eucaine solution. It is difficult to estimate the amount of pain suffered by the patients who are infiltrated. Some appear to have little or none, some more than they can bear. Vomiting and faintness, with much pallor, sometimes occur. Children are usually too frightened to submit to the process, and are upset by it. Some experience in the technique of the injection is needed to get a fairly good result, and the operator, unless accustomed to the altered appearance of the tissues caused by the infiltration œdema, will often be at a loss to distinguish structures as he meets with them in the course of his dissections. As a rule, picking up bleeding vessels causes pain. It is

recommended that patients should take a meal before the operation and be given alcohol, as it is said that it lessens the sensibility of the tissues.

"Dr. Leonard Corning, of New York, proposes yet another method of using cocaine. He first marks out with crayon the superficial veins, to obviate a possibility of puncturing them, and next exsanguinates the limb with an elastic bandage and Esmarch's cord. He then injects superficially three to five drops of a 1 or 2 per cent. solution of cocaine hydrochlorate immediately above the cord. After waiting until the skin is anæsthetic, he injects the deeper tissues with a solution of the same strength, making twenty or more punctures according to the area to be rendered insensitive. Dr. Corning then applies a tourniquet at the upper limit of the anæsthetic zone, and after a few minutes operates.

"This method assumes that cocaine, instead of being rapidly absorbed into the circulation, is by this method able slowly to permeate the tissues and exert its paralysing action upon the peripheral nerve endings.

"Dr. Bier punctures the sheath of the spinal cord in the lumbar region, and withdraws as much cerebro-spinal fluid as is considered necessary. He then injects with Pravaz's syringe cocaine solution, closing the puncture wound with collodion. The whole proceeding is rendered painful by infiltrating (after the manner of Schleich) the tissues down to the spinal column. He employed .5 per cent. or 1 per cent. solution, and found that 0.005 gr. cocaine or less injected into the sheath rendered the lower part of the body insensitive to pain. The after-effects recorded were slight, although in some cases troublesome."

The above passage is taken from the third edition of Dr. Dudley Buxton's work on 'Anæsthetics, their Uses and Administration.' The volume is one of Lewis's "Practical Series," and it is eminently practical, every form of anæsthetic having a place in it, and the varying circumstances under which each is best employed being definitely set out therein. The work has been thoroughly revised and brought up to date, and forms a most valuable handbook in its own particular branch of medical work.

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A DEMONSTRATION

ON THE

ELECTRIC LIGHT TREATMENT OF LUPUS VULGARIS.

By MALCOLM MORRIS, F.R.C.S.Ed., etc.,

At the "Light Studio," 1a, Berkeley Gardens, Campden Hill, W., on the 30th June, 1900.

GENTLEMEN,—I have asked you to come here to-day to see this new method of treatment of lupus. I have had the greatest possible difficulty in getting a place in London where I could put up the apparatus and carry out the treatment. I have had the apparatus more than eighteen months, and I got a room in a house and put the instruments into position at considerable expense; but, owing to the action of some of our own fraternity, I had to take the apparatus away because it was said that I was doing harm to the neighbourhood by carrying out the treatment, and I was threatened with an injunction in the Court of Chancery if I did not move away from there. At length I succeeded in finding this studio, which is the actual property of the landlord, and so I am not interfered with by anybody. The actual apparatus has been at work here for patients a little over two months, so that the statement which has appeared in the daily press and in a medical paper—the 'British Medical Journal' of this week,—that the London Hospital is the first to carry out the treatment, is not absolutely accurate. In addition to this having been in operation for considerably more than a month, my friend, Dr. Blacker, who is here, and will demonstrate some of his cases, has been treating patients longer by this method than I have. At any rate, both of us have been carrying on the treatment far longer than the London Hospital. The question of priority, of course, is of no real moment; the great point is as to the value of any particular treatment, and whether this form is of any use.

A word or two ought to be said as a preliminary about the methods of treatment we have been

in the habit of using for this disease—lupus vulgaris—for many years past. The first advance made in the treatment was when it was suggested that scraping should be done by Volkmann's spoon, and it was considered as a great triumph in surgery, as was the treatment of a tuberculous joint when the lupous tissue in this disease was scraped away. That was undoubtedly a distinct advance upon other methods of treatment, but it was exceedingly unsatisfactory, because in a large proportion of the cases there were relapses. There have been other methods of treatment which I need not go into now, chemical and otherwise, to remove the diseased tissue and prevent those relapses. It was urged strongly by the French school that we should not use any method which caused bleeding, because it was always felt that there was a considerable risk of the tubercle bacilli being transplanted through the medium of the blood. The French school, therefore, thought it was an advantage that the destruction should be brought about by cautery or by some application which would not cause bleeding, and that therefore there would be less risk of infection from one part of the skin to another. That was a distinct advance. But whether the destruction of the diseased tissue was brought about by means of the cautery or by acids, the fact still remained that every particle of the disease was not destroyed, and the disease relapsed again and again. It is this recurrence which has been one of the most unsatisfactory parts of the subject. It seemed, therefore, that sunlight or electric light, or some other method of treatment which was not violent in its action, might get at the bacilli in the deeper tissues, and so prevent these relapses. It must be stated that, so far as my experience of the light treatment is concerned, and so far as the patients I have treated here myself are concerned, as well as the cases I have seen at Copenhagen, we must not exaggerate the advantages of the new method. We must treat the whole subject in a tentative way, and say that, though there seems to be distinct progress made, we must not allow ourselves to make all sorts of exaggerated statements about the cure of lupus such as have recently been made. There are many points in connection with the treatment which have to be very carefully considered. First of all you have to consider the suitability of the patient, the suitability of the disease in the particular patient, and the particular part of the skin where

the disease is found. If there are parts of the body on to which it is extremely difficult to get the light focussed, as you will see in some of the cases I have to show you, the matter is a difficult one. Moreover, there is a marked difference in the tissue in the case of various patients. Another very important factor is whether or not the patient has been treated before by any other method. If the patient has had other methods of treatment which have resulted in the formation of scar tissue, that fact enormously increases the difficulty of bringing about a good result. If the situation is convenient for focussing the light the results are, in a large proportion of these cases, exceedingly satisfactory. It is satisfactory in many ways. First of all, it does not put the patient to any pain, or, if there is any pain, it is only very slight. Secondly, it does not require that any anæsthetic should be administered, as has been the case with violent operations in the past. There is nothing about the procedure to frighten the patient in any way. On the other hand, we have the fact that if the disease is extensive it is a tedious operation. Moreover, with the prolonged exposure the reactions tend to pass off; and this fact adds to the difficulties, because you do not, perhaps, get at the end of many sittings so deep a penetration of the light as you do at the beginning. Remember that the scar tissue and the pigmentation which are produced tend to limit the action of the light.

Before I demonstrate the cases to you, let me say a word about the technique. The patient lies upon a couch, as you see. A powerful electric light is fixed some six feet above the ground, and telescopes are fixed at some little distance from the light, to focus the rays upon the patient's skin as he lies on the couch.

The first patient, a man of about forty-five, has had lupus of the face for thirty-five years. He has suffered a great deal by treatment at various times, but the disease has always recurred. I show you some portions which are only of five years' duration, so that this part of the disease has not had so much treatment as the other parts, and is therefore more favourable for this particular treatment. He went to Copenhagen last year, and had a considerable number of exposures in the Institute there, under the direction of Professor Finsen and his assistants. After that course he came back to me so enormously improved, that that was one of

the first things which made me anxious to try the treatment myself. The whole of the portion I have shown you was one mass of apple-jelly tissue; there was not a single particle where the apple-jelly did not exist. He was in Copenhagen and while there had a considerable number of exposures, which made him enormously better. But during the winter there was a gradual relapse. Much of the diseased tissue was then removed, and much also left behind. For the last two and a half months, with the exception of two days and of Sundays, he has been exposed to the action of the light, one hour each day. His ear has not been treated by it at all. You will see the scar tissue. But he has also got a considerable amount of tanning or pigmentation. When I was in Copenhagen, at Whitsuntide, I saw sixty cases from which the lupous tissue was entirely gone, and the cases may be called actual cures. The time during which the treatment was taken varied, so one cannot state positively how long it takes. All these sixty patients had perfectly white supple scars, but around a considerable number of the areas was deep pigmentation, but this by degrees dies away. It is much the same result as that of exposure to sunlight. This patient is proceeding with the treatment and he will have the light applied very soon. It is only possible to treat at one time a certain limited patch, about the size of a half-crown. In order to cool the rays which pass down the telescope, a stream of water is kept flowing around the face-piece and then runs into the receptacle below. Sometimes, even with this precaution, there is considerable heat felt by the patient. It is very necessary that you should as far as possible remove the blood from the area about to be treated so as to get the pale surface. The pressure is a little uncomfortable to the patient. This is especially so in such positions as underneath the chin, as you can readily understand. It is also awkward near the eye, in which case the eye has to be covered by a pad, and a particular shaped glass is necessary for this awkward situation. The time the patient feels pain is when there is reaction. The amount of burning or pain seems different, not only in different individuals with the same disease, but in different diseases.

I have treated two other diseases here with marked results, which I will explain to you shortly.

Now, much of the success of this treatment depends not upon the doctor but upon the nurse. I do not think there would have been any possibility of getting good results in the short time that we have, but for this lady, Miss Olsen, who comes from Copenhagen, and who has been a nurse in the Institute there for a considerable time, and whose devotion to her work is very remarkable. It requires the greatest attention and patience on the part of the nurse to keep the glass accurately in position; because if the rays go to one side or the other much of the benefit of the treatment is lost. The treatment requires also the attention and perseverance of the patient and it wants a devoted and skilled nurse who understands how to carry it out. The square inch of surface selected for to-day's exposure is first of all lined round with a blue pencil, and on that area the exposure will be carried out for one and a quarter hour. At Copenhagen there is a large garden for the patients, where they can get the benefit of sunlight. Each one of these forty or fifty patients has his or her own nurse to hold the glass, and move its position as the sun's position alters in the heavens. You will see that the lens is fixed by means of this elastic band. At first, with a nervous patient, there is some difficulty in carrying out the treatment properly.

The patient I show you next has been treated by many physicians. She has had the Koch treatment, and many other forms of treatment besides. Further there is special interest about this case because, as my friend Dr. Harrison Low will tell you, she has been treated by X rays. As a matter of fact, we have mixed the treatment; that is to say, we have applied Finsen's electric light for some parts, and X rays for others. The involvement of the mucous membrane is a very serious matter in these cases, because there is a considerable risk of reinfection from the inside of the nose. Lupus is fond of the nose; it destroys all the soft tissues of the nose, and it will even go through the nose; and even if it does not do that, you are very apt to get reinfection from the inside of the nose to the outside again. That is one of the chief ways in which lupus may recur after apparent cure. For a considerable time she has had ulceration of the inside of the nostril. The application of the X rays to this part has resulted

in it becoming absolutely clear of the disease. That is a very important advance in treatment, for I know no other method, whether it be scraping, cautery, or the application of drugs, which has any real effect upon the mucous membrane of the nose. If the area involved is very minute, it is possible by means of the galvano cautery to remove the disease, but you cannot in the nose, where the mucous membrane is in folds. In Copenhagen they touch this part which is inaccessible to the electric light with iodine and other things, but without good result. We cover her face up with an ordinary mask of cardboard covered with lead foil, and cut out a piece where we want to apply the rays. Before she was put under the X ray treatment her nose was very much obstructed, but now she breathes through it quite comfortably. That obstruction of the nostril, oddly enough, was the condition which most troubled her, and it is the cure of that which she most appreciates. All the ulceration has also disappeared from the nose. There is another case also in which the patient had lupous ulceration of the gums and palate, and which was made very much better by exposure to X rays. In this case the patient opens the mouth for the light to impinge on the diseased parts, the rest of the face being covered with a lead mask. The exposures are for ten or fifteen minutes, and are carried out every day in the week except Sunday. The X rays are used for a varying time. Sometimes patients will come back and say nothing has happened in the way of reaction after treatment, and in that case we go on again. The reaction in some cases seems to be very similar to the reaction after the X ray application, but apparently it does not persist quite so long. The degree of reaction varies very much in different individuals. A high grade of tube is being used for the X ray application. The original tubes which we were recommended to use gave a spark of one and a half inches, but now Dr. Low is using a spark of six or seven inches. For the mucous membrane the X ray treatment seems to be sufficiently encouraging to persevere with, because the X rays reach portions which the focussed electric light cannot. Moreover, the exposure required is shorter in the case of X ray, and the expense is less. The one thing which can be placed against X ray application is a dermatitis which sometimes occurs, and which is difficult to remedy. With the X ray

the healing is more slow than with the electric light.

I show you now a very old and severe lupus of long standing. I have burnt it freely with the cautery, and it has had free applications of strong pyrogallie acid and nitric acid, with great advantage at the time, but the disease has always relapsed. When she began this treatment she was worse in every particular, both as to the extent and as to the character of the disease, than she has ever been before. The result has been in every way satisfactory. She has had about twenty applications, extending over four weeks, and the results are most encouraging. I show you nodules which will give you an idea what was the character of the general eruption before the electric light treatment was applied.

The present patient has got *not* lupus vulgaris but lupus erythematosus. It is difficult to know what is the pathology of lupus erythematosus. Some believe there are tubercle bacilli in it, others do not believe so. I am one of the latter. I show you the improvement which has been effected in her case in one area by one application. The reaction was immense so much so, that she wrote a letter afterwards to say that it was fearful, and she could submit to no more of the treatment. But she has since changed her opinion. Her husband, who is a doctor and is present, will tell you how bad it was, and how swollen and painful, but after about a week of this it quieted down, and as a result of the blistering from the application, you can see a wide white atrophic scar. In this disease, if there are no bacilli present to be destroyed, we are on different ground altogether. What has happened in this case of lupus erythematosus? It may be that there is a blocking of the vessels only. One saw in Copenhagen very remarkable results from the treatment in cases of simple *nævi*, that is to say, papillary *nævi*. There are no pathogenic bacilli in a simple *nævus*, yet a *nævus* will disappear, and a white scar will come in its place. Therefore there must be something more than the question of destroying the bacilli. There may be some chemical change, which may have an effect on the vessels, and so bring about such a scar. This opens up the possibility of treating other diseases by the same method.

Here is another case. This patient has been kind enough to come here to-day. It is a case of

very typical rodent ulcer. Not much has yet been done by this treatment before. When she first came to me she had a very typical cartilaginous edge to the lesion, with a depressed centre absolutely typical of rodent ulcer or epithelioma of the skin. It has been there five or six years. The reports about rodent ulcer were not particularly encouraging, but as this lesion was a small one, and exactly of the size to be covered by a glass, I thought I would give her a chance of trying. Otherwise I know of nothing except absolute removal, which has to be very free, so as to get outside the zone of diseased epithelium. But twelve exposures of one hour each at certain intervals have resulted in the greater part of the hard edge having disappeared, and it is now an ulcer which is slowly healing. Her last exposure was ten days ago. I consider that it is an exceedingly satisfactory result. If any hardness reappeared she could have further exposures. It seems to me that if only we can get rodent ulcer cured by it, it will be an immense stride. I have no photograph of the condition before this treatment, but you must take it that the appearance was absolutely typical. It had not ulcerated. It has been converted by this treatment into a superficial ulcer. I have another case of rodent ulcer under treatment at the present time, which is not sufficiently far advanced to show. There has been a very marked reaction, showing that reaction is not dependent on the question of organisms. This has been in existence nine years, during five and a half years of which the condition has been well marked.

Dr. BARRY BLACKER: It is very kind of Mr. Morris to allow me to say a few words about this treatment, which I have been carrying out for some time. There is not much to add to what he has already said, except to emphasize the fact that compression over the spot to be treated, so as to express all the blood, is of the greatest importance, whether you are treating *nævi*, or rodent ulcer, or tubercular disease. It is impossible for the ultra-violet rays to enter the tissues unless you get the red blood out of the capillaries, out of the path through which you wish to introduce the ultra-violet rays. Another point is the absolute necessity of keeping the glass fixed to the spot which you are treating, so that skilled assistance becomes an essential part of the treatment. I and

Mr. Morris have been extremely fortunate in having the assistance of Dr. Finsen's nurse, who has also been treating my cases. I do not know what we should have done without her.

This little girl has not been treated in any way except by light. She has been treated for two months, and I see her two or three times a week. There were six apple-jelly nodules in that spot when the treatment was commenced, and you will see that they are now practically gone.

Mr. MORRIS: The greater the amount of blood, and the more pigmented the skin, the greater the difficulty. I intend, as a pure experiment, to have an apparatus made with a spectroscope fitted so as to split up the rays and try to catch the violet and ultra violet rays.

Dr. BLACKER: Here is a case in which the mucous membrane of the nose and gums had been involved very considerably. The duration of the trouble is five or six years. The pressure on the gum was very painful, but the combination of the treatment of X ray and electric light has very much improved the mucous membrane.

Mr. MORRIS: When there is reaction after this treatment the patients are not really ill, like they are after the Koch method, but the part treated is sore. The softness of the scar and its suppleness is not the least important result of this treatment.

Congenital Lack of Nerve Structure.—

O. Heubner ('Berliner klinische Wochenschrift,' May 28th, 1900) describes a case of the variety to which Moebius has given the name of "infantiler Kernschwund." It is that of a child aged eighteen months, who was backward in respect to power of standing, walking, and speaking. In addition there were a lack of control over both abducentes of the eyeball, complete paralysis of the left facial nerve, diminished activity of those muscles supplied by the left motor oculi, a complete paralysis with considerable atrophy of the left half of the tongue, and a complete lack of tear secretion. There was also complete lack of response of the affected structures to both faradism and galvanism. The condition is referable to congenital structural defects in that part of the cerebro-spinal axis from which the nerves going to the affected areas arise.

Medical Record, June 23rd.

A CLINICAL LECTURE ON HÆMATURIA.

Delivered at Guy's Hospital,

By J. H. BRYANT, M.D., M.R.C.P.

GENTLEMEN,—In September last I had under my care in Miriam Ward a very extraordinary case of hæmaturia. The patient was a young healthy woman, twenty-one years of age. The hæmaturia came on quite unexpectedly after a severe shock to the nervous system. It was not accompanied by any other important symptom or physical sign, such as pain, pyuria, anasarca, or renal enlargement. The blood was intimately mixed with the urine and it had the appearance of having come from the kidney itself or from the pelvis of the kidney. After most careful analyses of the urine, which determined that blood was the only abnormal constituent, and after a thorough physical examination, which did not reveal anything abnormal, I came to the conclusion that we were dealing with a condition which has been variously named essential renal hæmaturia (Debairieux), renal epistaxis, renal hæmophilia, or hæmaturia from a healthy kidney. She was discharged apparently quite well. Twelve days after she was again admitted and remained in forty-three days, during which time she had pyrexia for a month, the temperature running a course very similar to that of typhoid fever. Then she developed a fresh urinary symptom which made the original diagnosis a little doubtful. I will read you an account of her condition during her first stay in the hospital, will then discuss the differential diagnosis of hæmaturia and give you a short account of essential renal hæmaturia; I shall then read you notes of her condition during her second sojourn in the hospital and discuss the diagnosis then made.

F—, twenty-one, admitted on September 6th, 1899, for hæmaturia. She has always been healthy and strong. She is married and had her first child in February last. It was an easy labour and she had no bad symptoms following it. Since May she has suffered from amenorrhœa and considers herself to be pregnant. There is no family history of tubercle or hæmophilia.

On August 15th she was much alarmed by some people running about and crying out "Fire!" She at first thought the fire was in the house adjoining her own and in consequence received rather a shock. When she next passed urine, she for the first time noticed that it was of a very dark colour, and it has continued so until the time of her admission. She is positive that she received no blow on her side. She has never experienced any pain on micturition.

Condition on admission:—Pulse, 72; respiration, 16; temperature, 100·4°. Tongue clean.

She is a well-developed healthy looking woman. There is no œdema and no pain.

Genito-urinary organs.—The os uteri was patulous, admitting the middle finger for about an inch. No tumour felt. Cervix not lacerated. Uterus a little larger and softer than normal. No pain or tenderness. Vulvæ normal. No urethral caruncle. Urine acid, specific gravity 1026. Albumen, blood, blood-corpuscles present, no casts. The blood was intimately mixed with the urine, giving it a uniformly red appearance.

Abdomen.—No distension. Kidneys not palpable. No pain or tenderness in the loins.

Bowels rather constipated.

Circulatory, respiratory, and nervous systems normal.

September 6th.—She feels quite well except for the hæmaturia. Extracti Ergoti Liq. 3ss every six hours was prescribed.

8th.—I saw the patient to-day and examined her thoroughly, but could not find anything to account for the hæmaturia. I thought she might be a case of so-called "renal epistaxis."

9th.—The amount of blood in the urine was much less.

10th.—There was no blood. She felt perfectly well, and was discharged.

The prominent, in fact the only important symptom of disease in this case was the passage of what appeared to be blood in the urine. Our first consideration was to determine whether we were dealing with a case of hæmaturia or hæmoglobinuria. Whenever you have a patient who appears to be passing blood in the urine your first care must be to determine which of these two conditions it is. Both in hæmaturia and hæmoglobinuria you can obtain the guaiacum and ozonic ether and Heller's (the liquor potassæ)

tests. In hæmoglobinuria blood-pigment only is passed in the urine, and it may be present in such large amounts as to colour the urine very deeply. In hæmaturia, in addition to the blood-pigment, red blood-corpuscles are also present. A distinction must therefore be made by means of a microscopical examination of the urinary deposit. If large numbers of red blood-corpuscles are present you are dealing with hæmaturia and not with hæmoglobinuria; occasionally you may come across a case of hæmoglobinuria in which a few red blood-corpuscles can be found in the urine. If there is a very large amount of blood-pigment and very few red blood-corpuscles the condition is probably hæmoglobinuria. I had a case of this nature under my care in 1898. The patient was a male æt. thirty-two; he was admitted on March 25th, 1898, for pain in the back and loins and the passage of dark coloured urine. He had been in the Army for seven years and had served in Ireland, Egypt, and Cyprus. Whilst in Cyprus he had a bad attack of ague. Since his return to England he had been subject to attacks of shivering during the winter months. For the two months previous to admission he noticed after a chill that his urine became very dark in colour. Five weeks before admission he had a severe chill and his urine became darker than he had ever known it before. After a fortnight in bed he was quite well again. On March 21st he caught another chill, had the usual pain, and the urine became dark; he was admitted for this attack. On admission the urine was found to be porter coloured, acid, specific gravity 1022. A spectroscopic examination of the pure urine showed the presence of the methæmoglobin band, and when the urine was diluted the two oxyhæmoglobin bands also became visible. Albumen five parts per 1,000 was present. A deposit obtained by means of the centrifugal machine on microscopical examination showed the presence of peculiar crystals, which were soluble in acetic acid but which were not identified, and "ghosts" of red corpuscles in fair numbers, but not at all in proportion to the amount of blood-pigment present. I discussed the condition and came to the conclusion that it was hæmoglobinuria complicated by slight hæmaturia. He came in again on November 19th and on the 29th. On both occasions the urine was porter coloured, but red

blood-corpuscles could not be then seen, so there was no doubt about the diagnosis of hæmoglobinuria, or to be more correct, methæmoglobinuria.

The presence of blood-corpuscles in the urine in some cases of hæmoglobinuria may be explained by the irritant effect of the disintegrated hæmoglobin on the tubules during its passage setting up an active congestion of the kidneys. In both conditions the urine may contain a considerable amount of proteid; in hæmaturia it is chiefly in the form of serum albumen, but in hæmoglobinuria it is mainly globulin. If specimens of the urine are saturated with sulphate of magnesia, globulin is precipitated but serum albumen is not.

The large numbers of blood-corpuscles found in the urine of the patient under consideration made us feel quite certain that it was a case of hæmaturia and not hæmoglobinuria. The next thing to do was, if possible, to determine the cause of the hæmaturia. Hæmaturia may be due to a large number of causes. It may be the result of morbid changes in the kidney, pelvis, ureter, bladder, urethra, or blood. It is most important for you to have some idea of the possibilities, in order to be in a position to discuss the differential diagnosis. The following is a fairly complete and classified list of the causes:

A. KIDNEY AND PELVIS.

1. Active congestion.

2. Congestion from poisons

{ cantharides.
turpentine.
copaiba.
cubebs.
carbolic acid.
hydrochloric acid.
oxalic acid, etc.

3. Passive congestion { heart disease. chronic lung disease

4. Nephritis { acute tubal. chronic tubal. chronic interstitial (at times) suppurative.

5. Calculi.

6. Embolism (especially in infective endocarditis).

7. Tuberculous disease.

8. Carcinoma.

9. Sarcoma.

10. Cystic disease.
11. Abscess.
12. Movable kidney.
13. Injury.
14. Parasites {
 - hydatid.
 - bilharzia hæmatobia.
 - filaria sanguinis hominis.
 - strongylus gigas.
 - pentastomum denticulatum.
15. Pyelitis {
 - acute.
 - tuberculous.
 - calculous.
16. Aneurysm opening into the pelvis.
17. From an apparently healthy kidney.

B. URETER.

18. Calculus.
19. Tubercle.
20. Growth.
21. Injury.

C. BLADDER.

22. Congestion.
23. Acute cystitis
24. Papilloma.
25. Malignant growth.
26. Tubercle.
27. Calculus.

28. Parasites {
 - hydatid.
 - bilharzia hæmatobia.

D. URETER AND PROSTATE.

29. Gonorrhœa.
30. Acute prostatitis
31. Calculus.
32. Injury.

E. HÆMORRHAGIC DISEASES.

33. Purpura.
34. Scurvy.
35. Hæmophilia.
36. Leuchæmia.
37. Hodgkin's disease.
38. Pernicious anæmia.

F. ACUTE FEVERS.

39. Malignant variola.
40. Malignant scarlet fever.
41. Remittent fever.
42. Cholera.
43. Ague, etc.

G. ACCIDENTAL ADMIXTURE OF BLOOD.

44. Menstruation.
45. Uterine fibroids.

46. Uterine carcinoma

47. Malingering (intentional addition of blood).

If the blood come from the kidney it is usually intimately mixed with the urine, so that it presents a uniform colour. The colour will depend on the amount of blood present, if there is a large quantity then it will be a dark red, if a small quantity a flesh pink or more probably a light brown (so-called smoky), which is due to the acid salts of the urine converting the hæmoglobin into acid hæmatin and methæmoglobin. After standing some time a dirty brownish deposit, resembling the sediment left in a cup of beef-tea may form: this is due to a deposition of phosphates which carries down the hæmoglobin and converts it into methæmoglobin. If the deposit is examined microscopically tube casts may be found, and their presence is an important indication of the kidney being the seat of the hæmorrhage. A small amount of blood and a large amount of albumen also points to a renal origin for the blood.

If the blood comes from the pelvis it is also intimately mixed with the urine. No tube casts are found.

If the blood comes from the ureter, long cylindrical clots, which are casts of the ureter, may be passed.

If the blood comes from the bladder it may be also intimately mixed with the urine; often, however, the first portions of the urine passed are quite clear and the blood only appears towards the end of micturition. Flat clots may be passed. If the blood comes from the urethra it may be passed at the beginning and at the end of micturition and the quantity is usually small.

From the points to which I have just drawn your attention it will appear that the condition of the patient's urine—the blood being intimately mixed with it—indicated the kidney or the pelvis as the seat of the hæmorrhage, and this was the conclusion we came to. We examined her carefully to make sure the blood came from no extraneous source. She was not menstruating, there was no evidence of uterine fibroids, uterine carcinoma, or of malingering. We could find no evidence of active congestion, a condition which may occur as the result of a chill or of an acute febrile disease. There was no evidence of the use of any

noxious drug either internally or externally. Hæmaturia may result from the local application of cantharides in the form of blisters. On looking over some of the old medical reports a few years ago I was struck by the fact of hæmaturia occurring not unfrequently as a complication of pneumonia. I found that in nearly all those cases large blisters had been applied to the chest, and it looked as if the hæmaturia had been caused by the blisters and not by the pneumonia, as it is a most exceptional complication of pneumonia since this particular form of treatment has been abandoned. If large quantities of turpentine had been taken the urine would have an odour resembling that of scented violets. Copaiba also gives a characteristic odour to the urine. Carbolic gives the urine a greenish-black appearance. After taking oxalic acid, large quantities of envelope-shaped crystals of oxalate of lime may be found.

There were no physical signs or symptoms of heart or lung disease, so that passive congestion was not considered.

There was no evidence of acute nephritis. In this disease the urine is scanty, of high specific gravity, and contains a large amount of albumen—far more than can be accounted for by the mere presence of blood. The total amount of urea is diminished, and blood and epithelial casts may be found on microscopical examination. General anasarca and pain in the loins are also usually present.

Chronic tubal nephritis may follow acute nephritis, the chief symptoms are anasarca, anæmia, headache, nausea, etc. The urine may contain a little blood, a large quantity of albumen, and epithelial granular and fatty casts.

In *chronic interstitial nephritis* hæmaturia is not a constant symptom. Patients suffering from this form of nephritis are liable to hæmorrhages—especially to epistaxis,—and occasionally a large quantity of blood is passed in the urine, the origin of which is probably the mucous membrane of the pelvis, and not the kidney substance. The urine is of low specific gravity, a large amount is passed in the twenty-four hours, and the amount of albumen present is small. The radial artery is generally thickened and tortuous, the pulse is of high tension and the heart is hypertrophied. It very rarely occurs in patients under thirty; when

it does, there is, in nearly all the cases, a previous history of scarlet fever. The youngest patient I have ever come across suffering from this disease was a girl aged nineteen. She died in the hospital. I performed the necropsy and found two very small kidneys weighing together only one ounce and a half.

Suppurative nephritis is secondary to diseases of the lower urinary passages, and the urine is generally alkaline and contains pus.

Calculi cause lumbar pain and attacks of colic; the pain usually starts in the lumbar region and runs along the course of the ureter. The urine, in addition to blood, may contain pus and crystals of uric acid, oxalate of lime, phosphates or cystin, the form of crystal usually corresponding to the composition of the calculus. Hydronephrosis, pyonephrosis, or perinephric abscess may result, and a painful and tender renal tumour may be felt. The absence of all these symptoms make a diagnosis of calculus unlikely.

Embolism occurs most frequently as a complication of infective endocarditis. This disease is characterised by pyrexia, anæmia, cardiac bruits, and signs of embolism elsewhere, *e. g.* enlarged and tender spleen. I have already mentioned that there was no evidence of cardiac disease, nor of any of the other signs of this disease, so that this condition was also excluded.

Tubercle.—Primary tuberculosis of the kidney causes as a rule lumbar pain, the passage of a large quantity of pus in an acid urine in which tubercle bacilli may be found, pyrexia, sweating, wasting, and a tender renal enlargement. A family history of tuberculosis may be obtained. None of these signs or symptoms were present.

Carcinoma causes lumbar pain, a renal tumour, intermittent hæmaturia and albuminuria. It rarely occurs in patients under forty years of age, and is associated with anorexia, progressive emaciation, anæmia and cachexia. The case was obviously not due to this cause.

Sarcoma may occur in patients of any age, and is frequently found in quite young children. It gives rise to a very large renal tumour and is associated with anæmia, emaciation and weakness. This condition was not considered.

Cystic disease may give rise to obscure uræmic symptoms similar to those associated with chronic interstitial nephritis. It is distinguished from

other conditions by the presence of a tumour in each lumbar region presenting the characteristic signs of a renal enlargement. The urine is similar to that found in chronic interstitial nephritis and the changes in the heart and the arteries are also similar.

Movable kidney occasionally causes hæmaturia. It usually gives rise to a dragging pain in the loin, and a movable tumour deeply situated in the loin, which gives rise to a sensation of nausea when palpated, can be felt.

Injury, to produce hæmaturia, must be of a severe character and the patient would be certain to allude to it. There was no injury in this case.

Parasites.—The microscopical examination of the urinary sediment in the case of hydatid might reveal the presence of scolices and hooklets; shreds of membrane may be visible to the naked eye. A renal enlargement may also be detected in some of the cases. If caused by the bilharzia hæmatobia the ova may be found on microscopical examination; they are egg-shaped bodies $\frac{1}{16}$ in. in length, and $\frac{1}{32}$ in. in thickness, presenting a terminal or lateral sharp spine. A ciliated body, which is the embryo, may be seen moving inside the ovum. In the case of filaria sanguinis hominis the urine is usually opaque and whitish or milky in appearance from the presence of chyle. Microscopical examination may also reveal the presence of embryo filariæ. Patients suffering from bilharzia hæmatobia or filaria sanguinis hominis have usually lived abroad.

Pyelitis.—Pus is usually present and there are no casts.

Aneurysm is extremely rare, and there was no cause for thinking it existed in the patient.

There was no evidence of it being due to any of the hæmorrhagic diseases. There was no other indication of hæmorrhage but the hæmaturia. There was no family history of hæmophilia. The spleen was not enlarged. There was no purpuric eruption. There was no anæmia. There was no reason to suppose it was due to any of the acute fevers. With the exception of the hæmaturia the patient was perfectly well. I have already indicated reasons for considering it was not due to any morbid condition of the bladder or urethra and so shall not discuss the diseases of these organs separately.

Excepting the hæmaturia the patient presented

no abnormal physical signs or symptoms, so I came to the conclusion that we were dealing with a case of hæmorrhage from a healthy kidney. I mentioned the case to Dr. Shaw, and he told me that the late Dr. Moxon was much interested in this condition and used to compare it to epistaxis. He argued in this way: you get epistaxis without any obvious organic change in the nasal mucous membrane, why should you not get hæmorrhage from the kidney in a similar manner? The sudden onset of the hæmaturia which occurred in such a healthy woman, but which was distinctly related to a severe nervous shock, suggests the possibility of it being due to some vaso-motor disturbance in the kidney, giving rise to a very active congestion of the organ.

It has long been recognised that renal hæmaturia may occur without any obvious morbid change in the kidneys and unconnected with any general disease such as purpura, hæmophilia, etc. It has come within the experience of most surgeons attached to hospitals to have operated on the kidney for hæmaturia, with the expectation of finding a calculus or some other morbid change, and to have found nothing at all to account in a satisfactory manner for the hæmaturia. Another curious feature of cases of this nature is, that, after exploratory nephrotomy and pyelotomy, the hæmaturia often ceases. Operations on other parts of the urinary tract have also been known to have been followed by a cure; Passet and Picquet, through a mistaken diagnosis, performed hypogastric cystotomy, and this operation was followed by cure. Hurry Fenwick, in the 'British Medical Journal' of February 3rd, 1900, describes two cases of this nature. One was a young woman, eighteen years of age, who had suffered from intermittent hæmaturia for five years. Attacks came on suddenly when she was in good health. A bacteriological and microscopical examination of the urine gave no indication of the cause of the hæmaturia. A cystoscopic examination was made, but no ureteral efflux of blood could be detected. A few weeks afterwards a cystoscopic examination was made when the patient was not expecting it, and jets of dark bloody urine could be seen coming from the left ureter. The patient was operated on and the pelvis was carefully incised and examined, and what appeared to be a villous

tumour of a papilla was found; the papilla was "bright red and fleecy," all the others appeared to be healthy. It was removed with a Volkmann's spoon and a subsequent microscopical examination of it was made by Mr. Targett, who reported that there was no evidence of growth, but that the vessels were congested and that there was extravasation of blood and an increase in the cellular stroma. He suggested that it might be an early stage of a fibromatous condition.

The second case was a lady, aged thirty. Hæmaturia had commenced fourteen days before without any apparent cause. With the exception of blood nothing abnormal was found in the urine. A cystoscopic examination showed blood to be issuing from the left ureter. This patient was operated on a week later and a "bright red varicose papilla" was found similar in appearance to the one described above. It was also microscopically examined by Mr. Targett, who found distended blood-vessels and extravasation of blood.

These two cases are of great importance, and help to clear up the nature of these obscure forms of hæmaturia. I cannot do better than quote the remarks of Fenwick on this point, he writes:—"I submit that in some, at least, of these cases, the bleeding emanates from one of the renal papillæ and the mucous membrane clothing it; that the papillary part of the Malpighian pyramid is engorged, and that its papilla is covered with a plexiform mesh of dilated vessels (varicosity of the papillary plexus). I suspect this vascular change is due to a localised patch of interstitial nephritis impeding the circulation in the boundary layer of the pyramid, and thus preventing venous drainage of the papillary plexus. It may be that the vessels are actually altered in the same way that the entire circulation becomes affected in the advanced stages of bilateral interstitial nephritis. Be that as it may, the site of the disease is apparent, and only apparent on opening the renal pelvis. A vivid red papilla assuming the aspect of a tiny villous growth stands out in strong contrast to the pale papillæ around."

In the 'Clinical Journal,' February 22nd, 1899, p. 303, an abstract of a paper by Debairieux on "Essential Renal Hæmaturia" is given. He described a case of hæmaturia of this nature. The patient, a woman twenty years of age, had suf-

fered from hæmaturia for six months, which began with an attack of influenza, and was accompanied by pain in the left side of the abdomen, the iliac fossa, and lumbar region. She had also attacks of pain resembling true renal colic. The urine was acid, the amount of albumen was exactly proportionate to the amount of blood, and there were no crystals, pus-cells, or casts. It was sterile. It was decided that the blood had a renal origin, and a diagnosis in favour of calculus was made. The kidney was explored and found to be normal. After the operation bleeding ceased. A second case was operated on by Broca. Hæmaturia had lasted five months. The kidney was found to be quite healthy, and after the operation there was no further hæmorrhage. A third case was operated on by Sabbatier, who diagnosed calculus. The kidney was removed; on examination it proved to be quite healthy. In all seven cases are described. The characteristic features are: that the hæmaturia may disappear spontaneously or after an exploratory operation, that no definite anatomical lesion can, as a rule, be demonstrated, and that it does not yield to medical remedies.

A theory that the hæmorrhage is due to an angio neurosis is put forward, a view supported by the fact of its occurring so much more frequently in women than in men, and that it may be cured by operations other than those performed on the kidneys. The occurrence of the hæmorrhage after a severe nervous shock also supports this view. Fenwick's two cases in which the abnormal papillæ were found are of the greatest importance, and in future when such cases are operated on the pelvis should be explored and abnormal papillæ looked for.

I will now read you notes of the condition of the patient when she came into the hospital for the second time.

September 22nd.—She was readmitted for hæmaturia. She continued quite well until the 16th, when the hæmaturia reappeared in spite of the fact that she had been lying up and doing very little work. On the 20th she was seen in the front surgery but could not be admitted. Tr. hamamelidis was prescribed. She returned on the 22nd and was then readmitted.

Condition on arrival.—Except for a systolic bruit in the pulmonary area her condition was the same as before. The abdomen was not distended,

there was no pain or tenderness, and no tumour could be felt in either of the lumbar regions. On deep pressure in the hypogastric region a lump, which appeared to be the uterus, could be felt, and on deep pressure she complained of a little pain there. The urine contained a large quantity of blood. Ergot was again prescribed.

September 24th.—There was no diminution of the hæmaturia.

25th.—Some urine was drawn off with a sterile catheter.

The urine was equally coloured and contained a large quantity of blood. On bacteriological examination it was found to be sterile.

27th.—At 9 a.m. she passed some very smoky looking urine. At 1 p.m. she passed some more urine which was quite clear.

28th.—No blood could be detected by the Tinct. Guaiaci test.

30th.—She passed sixteen ounces of clear urine.

During the afternoon of October 1st the temperature rose to 102.4° and she complained of slight sore throat. The urine was clear, of acid reaction, and some corpuscles were seen under the microscope. No hæmaturia.

October 2nd.—Temperature, 102.8° ; pulse, 116; respiration, 15.

3rd.—Temperature, 103.6° ; pulse, 112; respiration, 28. She complained of pain and also tenderness in the right iliac fossa. No blood in the urine, which was 1020, acid, no albumen.

4th.—Temperature, 101° ; pulse, 100; respiration, 20. No hæmaturia.

9th.—She was again examined, and it was suggested that the hæmaturia was caused by commencing tuberculous disease of the kidney. The tonsils were inflamed.

11th.—A cultivation was made from the tonsil for bacteriological examination.

12th.—Staphylococci were found in the culture; there were no Klebs Loeffler bacilli present. No blood in the urine, which was acid and pale.

14th.—Urine, 1020, clear, no albumen or blood, slightly acid.

18th.—Pus-corpuscles were seen on examining the urine microscopically. The deposit was stained for tubercle bacilli, but none were found.

21st.—Pus was still found in the urine.

23rd.—The urine was dark coloured and contained a good deal of pus but no blood.

25th.—A specimen of urine was drawn off by catheter this morning. It contained a good deal of pus and some albumen but no blood. It was thought that the pyuria might be due to cystitis so the patient was given Urotropine gr. viii, t.d.s.

28th.—Urine clearer, only a few pus-corpuscles visible.

November 3rd.—The report states that she was quite well, and she left for a convalescent home.

The two most important symptoms to explain were the pyrexia and the pyuria. The temperature commenced to rise on September 23rd, when it reached 99.4° . On the 26th it was 99.8° , on the 28th, 100.4° ; on the 30th, 101.4° ; on October 1st, 102.4° ; on the 2nd, 103.6° ; and from this day until October 23rd it ran an irregular course varying from 102.8° to 97.2° . From October 23rd to November 3rd, the time of her discharge, it was normal. No definite diagnosis of the cause of the pyrexia was made. She complained of a sore throat, and on examination her tonsils were found to be red and swollen; cultures were made, but staphylococci only were found. It was thought that the tonsillitis would hardly account for a month's pyrexia. She also complained of pain in the right iliac fossa, and appendicitis was thought of. If I had been asked to express an opinion on the nature of the disease from an inspection of the temperature chart only, I should have said that it looked very much like typhoid fever. This possibility was considered but not entertained. Pyuria was first noticed on October 18th. A catheter was first passed on September 25th; on October 14th the urine was noted as being faintly acid. What was the origin of the pus? It did not seem likely that it could have been the result of the catheterisation twenty-two days before. The question of the possibility of the pus not coming from the urinary tract at all was considered and another catheter specimen was obtained on October 25th. It was found to contain pus, which disproved effectually the view that it might have accidentally reached the urine after it had left the urethra. The hæmaturia, pyrexia and pyuria suggested a diagnosis of tuberculosis of the kidney.

No renal tumour could be felt and no tubercle bacilli were found. The inability to detect the presence of tubercle bacilli in a urinary sediment does not necessarily prove that the cause of the

pyuria is not tuberculous disease of the kidney. I have now seen several cases of tuberculosis of the kidney in which, after repeated examinations, no tubercle bacilli could be found in the urinary deposit. Guinea-pigs, however, inoculated with pus obtained by centrifugalising the urine have subsequently succumbed to tuberculous disease. No inoculation was made from the pus of this case.

Cystitis was thought of as a possible cause, and on October 25th Urotropin gr. viii was administered three times a day. This is a drug which is excreted by the kidneys as formalin, a strong antiseptic and bactericide. On the 28th only a few pus-corpuscles could be seen on microscopical examination; there was no naked-eye deposit.

The pyuria might be explained by typhoid fever, as pyelitis is an occasional complication of this disease.

The temporary nature of the pyuria and pyrexia, the absence of any renal enlargement, pain, or tenderness, the well-nourished condition of the patient, and her good family history, were all points against tuberculous kidney being the cause of her trouble. The obscure nature of the symptoms during her second sojourn in the hospital has made the diagnosis of the cause of the hæmorrhage still more difficult and unsatisfactory. I thought a lecture on this case would be instructive and useful to you, as it has brought in so many important points in connection with the differential diagnosis of hæmaturia.

[May 1st, 1900.—The patient came to the hospital last week. She was the picture of health. Since her discharge she has passed no more blood in her urine.]

Puerperal Mastitis.—Brouha reports such a case on account of its occurrence at a period soon after labour. The first symptoms were noticed within twelve hours after delivery, and before the child had been nursed. No causative lesion of the breast could be found, and no infection of the genital tract was present. The writer attributes the mastitis to the efforts before labour to obtain perfect cleanliness of the nipples, during which the germs may have entered the breast through some solution of continuity of its surface.

Amer. Journ. of Obstet., June, 1900.

ON DIAGNOSIS BY EXCLUSION.

BY

ALFRED S. GUBB, M.D.(Paris).

ONE of the principal difficulties in arriving at a correct diagnosis, especially for the young practitioner, is the tendency to form an opinion as to the significance of the symptoms without running over in his mind the various possible explanations. Once he has prematurely made up his mind, however, he is apt to allow his judgment to be warped by the dominant idea, and either to neglect details which do not fit in with his hypothesis, or to strain them into conformity. Later, as the result either of more detailed observation, or thanks to "another opinion," the scales fall from his eyes, and he wonders in his mortification how it was that he overlooked the now obvious deductions to be drawn from the facts at his disposal.

The first step on the path of trustworthy diagnosis in each individual case is to eliminate the graver possibilities one by one; and not until this has been done is it safe to fall back upon the simpler and less alarming diagnosis. If, for lack of the necessary data, this process of exclusion is not for the time being practicable, the prudent practitioner will suspend his judgment, and will merely state that, while it may be this, that, or the other, he is not yet in a position to arrive at a definite conclusion. By not jumping to conclusions, and by the methodical application of this method of "diagnosis by exclusion," the practitioner will save himself from many an error, and from much discredit and mortification.

Let us take, for instance, a patient who complains of sore throat. The fauces are injected, and swallowing is painful. There is more or less fever, and there are the usual indefinite aches and pains all over the body. The patient may not look very ill, and after a cursory examination nothing is easier than to declare it to be a case of simple tonsillitis, and to prescribe accordingly. If the precaution be not taken to notice the appearance of the tongue and the colour of the skin of the face and about the root of the neck, the existence of scarlet fever may readily be overlooked, and a correct diagnosis be made, if at all, only when desquamation takes place, or some kidney trouble

excites suspicion. If the throat be not carefully explored, and due note taken of the severity of the constitutional disturbance, a diphtheritic patch, small in area at this stage, may escape detection, and a dangerous centre of infection be left at large and untreated. But sore throat is also a symptom of syphilis; and on many occasions this (if properly understood) valuable symptom has been misconstrued, and the patient's recovery delayed in consequence. Now each of these conditions is, as a rule, easy enough to distinguish if only the possibility thereof cross the mind of the observer. The possibility of syphilis can, as a rule, be put on one side forthwith, by reason of the nature of the throat lesions, the age of the patient, and certain social considerations, though the last-named should not be accorded too much weight, seeing that syphilis is no respecter of persons. If circumstances leave room for doubt, methodical search for other symptoms and points in the personal history and habits of life will often clear up any lingering doubt. In the same way, unless very early in the case, scarlet fever and diphtheria can be eliminated, leaving us at liberty to seek a simpler explanation of the appearances.

Abdominal pain of one kind or another is a symptom of very common occurrence. It may be merely due to flatulence, or it may, when inquired into, prove to be the manifestation of this or that grave internal lesion. Before we arrive at the conclusion that the pain is of the simple kind we must consider its situation and severity. It may be that on investigation we find it starts in—if, indeed, it is not limited to—the region of the cæcum. If so, we have to consider, *inter alia*, the possibility of its being due to appendicitis or to typhoid fever. The differential diagnosis between these two conditions is by no means always easy; but if we can narrow the scope of possible diagnosis to these two affections, an important point will have been gained. In typical cases of abdominal pains in the "right hand bottom corner," associated with fever, diarrhoea will weigh in favour of typhoid, while the history or existence of constipation would argue in favour of appendicitis. I need hardly remark that in every case of diarrhoea the practitioner ought to bear in mind the possibility of typhoid. This is a very insidious disease, and no two cases start alike. I can recall, not without some compunction, that in my younger days there

was hardly a disease which, at one time or another, I have not diagnosed in seeking to explain the initial symptoms of typhoid fever, and this simply because the idea of typhoid did not cross my mind. Nor should the absence of diarrhoea be allowed to put the practitioner off his guard, because, though usual, it is not a constant symptom—during the first two or three days, at any rate. A furred tongue, with comparatively high fever and headache, with troubled slumber and bronchial catarrh, should lead the physician at any rate to review the possibilities. If the pain be very severe, and associated with belly-walls of plank-like hardness and more or less collapse, the history of the patient, if carefully inquired into, may raise the question of rupture of a gastric or duodenal ulcer, in deciding which the presence or absence of liver dulness will prove of assistance. If the patient be a female, and the pain intermittent, abdominal palpation should be resorted to for the purpose of eliminating the possibility of the pains being due to impending abortion, or even labour; for I have seen labour pains at term in a single woman misinterpreted, to the detriment of the practitioner's reputation and peace of mind. We must never overlook the possibility of strangulated hernia as a cause of abdominal trouble, and in children intussusception is a contingency to be borne in mind; mucous, possibly blood-stained, diarrhoea should always lead the practitioner to search for an abdominal tumour. In every case of abdominal pain, indeed, no diagnosis should be attempted without having recourse to abdominal palpation, which often yields the most unexpected results. It does not take long; by constant repetition it gives the physician the *tactus eruditus*, and it averts the likelihood of his overlooking a condition of things which is perfectly obvious when attention is directed thereto. It has been said with reason that mistakes in medicine are far more frequently due to carelessness than to ignorance, and this is the key-stone of my remarks.

Passing on to another class of cases, we often meet with patients suffering from what they perhaps describe as bronchitis or asthma. There is manifest shortness of breath, with râles or sibilant rhonchi on auscultation. The hurried or careless practitioner calls it chronic bronchitis or emphysema without further ado, and prescribes accordingly. Now the existence of shortness of breath

or "asthmatic" symptoms, ought always to suggest an examination of the heart and the urine. Over and over again I have met with cases in which methodical examination unexpectedly revealed a hypertrophied heart and albuminous urine, demonstrating the fact that the dyspnoea was due to incipient uræmia. In the same way I have seen so-called hysterical fits in pregnant women turn out to be due to eclampsia, and noisy respiration, attributed to a growth supposed to be present on the vocal cords, prove to be due to œdema of the larynx, associated with advanced kidney disease. When we have eliminated kidney disease, including diabetes (not because it has any relationship to the kidneys, but because it is indirectly an occasional cause of dyspnoea) and heart disease, we may turn our attention to the lungs. Even though it is often possible to detect pulmonary lesions sufficient of themselves to account for the dyspnoea, this fact does not dispense with the necessity for ascertaining the condition of the heart and kidneys. These suggestions may appear trivial, but it is only by such methodical elimination that we guard against the occasional error, which injures a practitioner's reputation to an extent quite out of proportion to the frequency of its occurrence.

It is hardly necessary to point out that, though aphonia may be due to "cold," it is in a tangible proportion of instances associated with the presence of an aneurism of the aorta, to hysterical paralysis of the vocal cords or cord, or to some toxic factor, inducing peripheral neuritis, such as lead-poisoning and the like.

Skin eruptions are a fertile source of error in diagnosis. Whenever we meet with an eruption which can by any possibility be suspected to belong to an exanthematous disease, the strictest inquiry should be made into the history of the case, especially as to the date of its appearance in relation to the onset of the initial malaise. I remember an instance in which I was called to a gentleman suffering, according to his own statement, from a recurrence of renal calculus. He had intense pain in the back, and some fever. It was late at night; the history was tolerably circumstantial, and I incautiously fell in with the suggestion. He was much better the following morning, and was getting ready to leave for the Continent, but he asked me casually to prescribe for a rash which had made its appearance on his face. He had then no fever,

and I neglected to push my inquiries further. It turned out to be a case of measles, and great was my subsequent mortification, as may well be believed. In another instance a servant was sent to the hospital, of which I was the resident medical officer, with a note from her mistress stating that she had fallen from the dresser a day or two previously and had injured her back. She was evidently very ill, with a temperature of 104° F., and complained of great pain in her back where she had been struck. Careful examination failed to reveal the slightest trace of injury in that situation. I noticed that the backs of the wrists were rough, but paid no particular attention to the fact, and admitted her under observation into a surgical ward. The next morning it became obvious that she was suffering from smallpox. I should like to mention another case—one of considerable public importance. In the course of conversation with a chance acquaintance he happened to express in very strong terms his horror of vaccination, and on my inquiring the cause he informed me that his youngest child, a baby four months of age, was dying from the effects of vaccination with impure lymph. I questioned him, and he stated that there could be no doubt as to the cause, which, moreover, had been admitted by the doctor who performed the operation. I asked permission—possibly somewhat irregularly—to see the child as a matter of curiosity. Though, like most other medical men, I had had little experience of irregular vaccination rashes, I felt convinced, from my knowledge of skin disease in general, that the ulcers with which the little sufferer was covered—some of them large enough to lay the finger in—were due to an entirely different cause. I at once thought of medicinal rashes, but was met with the difficulty that the infant had been perfectly well before vaccination, and had never been given medicine of any sort. A day or two later I brought up the subject in the presence of the mother, and I asked her if she herself had been taking medicine. It turned out that she had been taking 15 grains of bromide of potassium three times a day for a month or so, and though she herself had not developed any rash, the analysis of her milk revealed the presence of bromine; and on her discarding the medicine the child made a slow but perfect recovery. Unfortunately this case had already been published in a daily paper as an instance of "vaccination poison-

ing ;" and although I took the trouble to communicate the correct version of the case, the editor, in the exercise of his discretion, dropped my letter into the waste-paper basket. This is an instance of the methods adopted in the preparation of anti-vaccination statistics.

My object in relating these cases is to emphasise the necessity for extreme caution in deciding as to the nature of an eruption. Above all one ought to avoid the hackneyed diagnosis of eczema unless the eruption conforms to the classic description of that dermatosis. Eczema is a refuge for the destitute in the matter of skin diagnosis. True it is so common that in two cases out of three the diagnosis may possibly be correct, but one error in three, or even in ten, is a serious matter. In considering the diagnosis of eruptions which are manifestly not the outward and visible manifestations of an exanthematous fever, and which, moreover, do not fall into the group of eczemas, we are often confronted with dry, more or less circular, scaly patches, and we think of psoriasis. If similar patches are not present on the spots usually invaded by psoriasis, such as the elbows and knees, the possibility of their being syphilides should suggest itself. The diagnosis is usually easy enough once suspicion has been aroused, but we must not forget that the two may exist coincidentally, each complicating the other. The suspicion of a syphilitic taint should be excited by abnormality in the appearance or distribution of the patches, and inquiry into the past history will often make matters clear. A still commoner error is to confuse a scaly, seborrhœic eczema with psoriasis. The great thing to bear in mind, whenever an eruption does not conform to any well-recognised form, is that it may be of medicinal origin.

As we are constantly being reminded by the advertisements of purgative nostrums, constipation is the commonest ailment which flesh is heir to. Before deciding in our minds that obstinate constipation is due merely to functional indolence it is well to consider the possibility of its being caused, in children by intussusception, volvulus or hernia, and in adults by hernia, typhlitis, or the pressure of a malignant growth. In elderly people especially the existence of inveterate constipation ought always to suggest the propriety of enquiring as to the size and shape of the motions, and unless the replies are satisfactory exploration of the rectum is

indicated. In more than one instance it has been my good fortune to discover the existence of a rectal growth calling for operative measures by acting on this principle.

In so-called rheumatism of one joint, especially if that joint be the knee, the possibility of the affection being due to gonorrhœa must not be overlooked.

There is one other point which I think ought not to be passed over, viz the various causes of loss of consciousness. Apart from traumatism, loss of consciousness may be due to a "faint," or to hysteria. Tremulousness of the eyelids and sensitiveness to pain or discomfort will serve to identify the latter, but we have to distinguish mere syncope from coma, associated with uræmia or diabetes, from the post epileptic state, from apoplexy, and lastly, from narcotics. No history may be obtainable, so we have to base the diagnosis on the appearances. First and foremost is the condition of the pupils, whether equal, whether dilated or contracted, and whether they react to light. Inequality of pupils is suggestive of apoplexy, or, at any rate, of a one-sided cerebral lesion. Pupils contracted to a fine point are almost pathognomonic of poisoning by an opiate. Widely dilated pupils are met with in post-epileptic and alcoholic conditions, in which case they respond lazily to light, or the dilatation may be due to mydriatic drugs, such as belladonna, and, if so, they do not contract on the approach of light. If the diagnosis be still doubtful the odour of the breath should be noted—uriferous in uræmia, sweet (acetone) in diabetes, and *sui generis* in alcoholism and poisoning,—and the urine examined. A patient in whom unconsciousness is attributed to alcohol should never be left until the diagnosis has been confirmed by restoration to consciousness, because an overdose of alcohol does not preclude the possibility of other causes of insensibility, such as fractured skull, for instance.

'Les Nouveaux Remèdes' of February 8th, 1900, gives the following prescription for menstrual pain :

Codeine	1 gr.
Chloral	15 gr.
Bromide of ammonium	15 gr.
Camphor water	1 oz.

One quarter to one half of this preparation may be given before retiring at night.

Therapeutic Gazette, June 15th.

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A CLINICAL LECTURE.

Delivered at the London Hospital,

BY

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GENTLEMEN,—To-day I am going to speak about some cases that are or have recently been in the ward.

The first case is one of the chronic renal disease peculiar to pregnancy. Two kinds of renal disease are peculiar to pregnancy. This chronic renal disease peculiar to pregnancy occurs, as far as we can judge, in about one pregnant woman in every hundred. By itself it is a trivial thing; its importance consists in the fact that it renders the patient liable to the graver renal disease which causes puerperal eclampsia.

This patient is aged twenty-three, has been married eleven months, and was eight months pregnant with her first child. Her feet began to swell more than two months ago, and the œdema gradually increased. Two or three weeks ago there was aching in her back and sides, and she could hardly bear to lie down, and, in addition, she had been vomiting. She did not sleep well, but she had no headache. She had had no fits. She had been passing very little urine lately.

When she came in there was some œdema of the legs, buttocks, back, and abdomen, but not of the vulva or face. The uterus reached a hand's breadth above the umbilicus, and the foetal heart was heard. There was no retinitis.

This is a fairly characteristic case of the chronic renal disease of pregnancy. The symptoms are slight, so slight that often the patient can give no definite date as to the time at which the affection began. In this case, although there was œdema of the feet it did not affect the genital organs. The œdema is seldom great; it generally affects the lower extremities, but it is not always limited to them. These patients gene-

rally complain of weakness, often of headache, which this patient had not; and vomiting, which this patient had; and they are generally anæmic, which this patient is. The urine contains a considerable quantity of albumen—one quarter to one half its bulk. This patient, when admitted, had one half albumen in the urine. The symptoms I have mentioned, with loss of appetite added, made this patient seek advice. She had noticed that the quantity of urine had been diminished lately, and that is the rule in these cases. This kind of disease is only met with in pregnant women. When a woman is delivered recovery is rapid, the albumen disappearing within a week. This patient has been delivered nine days, and the amount of albumen has sunk to one eighth. When a patient is delivered, there is great diuresis, the diuresis being in proportion to the diminution of the urine before the delivery. Before delivery, not only is the quantity of urine diminished, but it is of a lower specific gravity than normal, and contains less urea. After delivery, with the increased flow of urine there is a greater proportion of urea. In this case the bowels have been loose, and therefore it has been difficult to record the quantity of urine passed, and the figures I read to you do not represent the whole amount. The day after delivery more than thirty-two ounces were passed in the second half of the day. Five days after delivery the quantity was eighty-three ounces in one day. On another day there were eighty ounces of urine passed, of a specific gravity of 1016 and containing .8 per cent. of urea.

The average age of patients with this disease is rather above that of the patients who suffer from eclampsia, and the disease is as common in subsequent pregnancies as in the first. Eclampsia is more common in the first pregnancy. The importance of the condition is that about one in five of these cases are attacked by the acute and dangerous disease known as eclampsia. The treatment of this disease is important chiefly in so far as it tends to ward off puerperal eclampsia. The treatment consists in the first instance of two things: (1) absolute rest in bed, and (2) milk diet. Milk diet means that the patient is kept on milk and nothing else. I will not trouble you with any theories as to why it is so, but it is a fact that if you put the patient on a meat diet the

amount of albumen will increase, while if you put the patient to bed and on a milk diet the quantity of albumen diminishes. If under such treatment the quantity of albumen does not diminish and the symptoms do not improve, then the best course is to induce labour with as little delay as possible. Labour should not be induced without first trying the effect of a milk diet, because it will sometimes cure the patient. I have seen patients with this disease who have been put on milk diet, and the result has been that the albumen has diminished, the patients have gone to full term and been delivered of living children. If rest in bed and a milk diet do not improve the patient, there are two reasons for terminating the pregnancy. The great reason is the liability to eclampsia, which is dangerous both to the mother and to the child. The lesser reason is that it has been shown that in pregnancy with renal disease intra-uterine death of the child is more frequent than in healthy women. In this woman the case was complicated by having a high temperature, which seemed to make the case more urgent, and for that reason labour was induced at once. The temperature went down the next day; it was due to some temporary and accidental cause which was not discovered. The child was born alive and weighed 3 lb. 3½ ozs. Since then the urine has increased, the albumen has decreased, and I expect the patient will go out well. In these cases the albumen is often composed in large proportion of paraglobulin. That is supposed to be because the paraglobulin transudes more easily than serum albumen. The presence of a large amount of serum albumen is supposed to indicate graver disease of the renal cells. This case is exceptional in that the albumen is not paraglobulin. Though I give you that as the general rule, I confess that the cases in which the point has been investigated are too few to admit of our saying how numerous the exceptions are.

I come now to two cases illustrating a different matter. One is that of a patient who is in a most deplorable condition. She has a great gap at the base of her bladder, measuring one inch and three quarters in length, and an inch to one inch and a half in breadth. A gap of that size could be easily repaired if the bladder could be drawn down to the vulva. But in this case the

vagina and the base of the bladder are fixed by organised fibrous exudation the result of pelvic cellulitis, so that it is no exaggeration to say that the margins of this fistula are as hard as wood, and it will be absolutely impossible, in her present state, by any sort of operation, to bring the margins of this fistula together. I now speak of this condition from the point of view of its prevention, because it might have been altogether prevented. I do not say that it is the fault of her medical man, because he very likely was not able to do what he would have wished. He probably did not have the opportunity of seeing the patient before delivery; I know not at what stage of labour she sent for him. She says she was quite well till her child was born four weeks ago. The child is said to have been dead ten days before delivery. Delivery was performed with forceps during anæsthesia, and the child is said to have been decomposed. The patient has some anæsthesia and atrophy of the muscles of the legs, which is due to pressure on the pelvic nerves during delivery. She has a small pelvis. I do not know its precise size because it is filled up with hard organised inflammatory exudation, but judging by the external measurements, and by feeling through the abdominal wall I have no doubt the pelvis is contracted. The child had been dead ten days before delivery, and if the doctor had been sent for early in labour and had recognised that the child was dead, the proper thing for him to have done was to have perforated the child at once, and not to have used forceps. I think in this case the difficulty was probably due not only to a small pelvis, but to a large child, because of the atrophy of the muscles of the leg, and the large size of the slough which was separated from the base of the bladder. The slough at the base of the bladder might have been produced by pressure of the head on the brim of the pelvis. But this pressure on the sacral nerves leading to wasting of the muscles and to anæsthesia could not have been due to arrest of the head at the pelvic brim; it indicates prolonged and great pressure while the head was in the pelvic cavity. It points to there having been a labour in which the head had got into the pelvic cavity and had been arrested there a long time. The patient, when in labour, should have

been delivered by perforation without wasting time in attempting forceps delivery. But then comes the question why the child died, and whether its death could have been prevented. It is, unfortunately, not at present customary for women when they engage a doctor to attend them in their confinement, to go to their doctor or invite his attendance until taken in labour. If difficulty in labour is to be prevented, the pregnant woman ought to be examined when seven months gone, and again, if necessary, at eight months, and the object of that examination should be to ascertain the *relative* size of the pelvis to that of the child. If this patient had been examined when seven or eight months gone, it would have been found that the child was large and the pelvis contracted, and either the labour might have been induced, or the patient delivered in a way which would not have entailed such damage as she has sustained.

The ways of ascertaining the size of the child during pregnancy are, in the first place, by absolute measurement, and then by feeling whether the head can be pressed into the pelvis. The latter is the best of all ways, because it does not matter how large the child's head is if the pelvis is large also. If the pelvis is small an average child's head would be large in proportion to the pelvis. To determine this, put the hands on the abdomen above the equator of the foetal head and press the head down into the pelvis. If the pelvis is contracted with relation to the head you will find the equator of the head above the pelvic brim. If you can push the head down into the pelvis labour will not be mechanically difficult. There are cases in which the head does not present, and in such cases you have to go upon other measurements. The average greatest circumference of the abdomen of a woman at full term of pregnancy with a child of average size, and without excess of liquor amnii, is thirty-five to thirty-six inches; the latter size is easy to remember because it is just a yard. I think it is a good rule, when a patient asks you to attend her in labour, to advise her to measure her circumference, and if it is more than thirty-six inches she should come for examination. If her circumference should exceed thirty-six inches it does not follow that the child is of excessive size, because there are other things which affect

her girth, such as the amount of fat on the abdominal walls, her muscular development, and the size of the pelvis. If she is a big, fat, muscular woman she will measure more than thirty-six inches. You must allow for these circumstances before drawing inferences. If there is a great amount of liquor amnii the girth may be greater without there being a child of excessive size. But, even if a patient has a greater girth than thirty-six inches, and there should not be found necessity to interfere, no harm is done in examining her abdomen. Another method is to measure the length of the uterus. It has been found that the length of the uterus is about half the length of the child, so that if the length of the child is twenty-one inches, the length of the uterus will be about ten inches and a half. But this measurement depends partly on the liquor amnii present and partly on the position of the child. I think the relation is not close enough to be an absolute guide. When you cannot feel the foetal head and judge whether you can press it into the brim of the pelvis, the other two measurements—the girth of the patient and the length of the uterus, are useful. There have been attempts made to ascertain the child's size in other ways. One ingenious gentleman proposed to measure the child's foot and judge the length of the child by the length of its foot. But if the os uteri is so far open as to enable you to feel the child's foot, the question is settled whether labour should be induced or not. Another suggestion is to measure the length of the sagittal suture; but here again the same thing applies; to enable you to make the measurement the labour must have begun.

Another case now in the hospital is that of a patient who came into the hospital in June of last year. This patient had been attended by a doctor outside the hospital, who had used forceps. He had pulled but had not delivered the child. Failing to deliver with forceps he sent the patient to the hospital. When she was admitted the os was half dilated, the cord was felt running across the os uteri, and as it was not pulsating the resident accoucheur knew the child was dead. He therefore perforated and delivered the child. The patient made a good recovery and went out of the hospital quite well. Her pelvic measurements were: interspinous

measurement, nine inches; intercostal, ten inches and a half; external conjugate, seven inches; true conjugate, three inches and three eighths. Now, the average length of the true conjugate is from four to four and a half inches, so this pelvis was distinctly contracted. We have, unfortunately, no record of the size of this first child, except that the patient says that it was not larger than usual. My conjecture is that the reason the doctor failed to deliver with forceps was that he put the forceps on before the os uteri was dilated, and that he got tired before he had completed the dilatation of the os uteri. Forceps ought not to be used before full dilatation of the cervix except in very special circumstances. Prolapse of the cord was probably due to the contraction of the pelvis, and the presence of prolapse of the cord ought to have caused a suspicion that the pelvis was contracted. If I had been called to a labour of that kind, with a contracted pelvis and the cord prolapsed, I think I should have turned the child and delivered it by the feet. The patient was told to come, in her next pregnancy, to the hospital, and she did so. She last menstruated at the end of February, 1899. She came to the hospital according to advice, as she was anxious to have a living child. It was intended that if delivery did not take place by the natural efforts that the pelvis should be enlarged by symphysiotomy. Labour began on December 4th, and she was delivered naturally of a child weighing 6 lb. 15 ozs., or only one ounce short of the average weight. This case shows how large a margin there is in a normal pelvis for the easy delivery of a child of average weight. Here is a considerably contracted pelvis—three inches and three eighths. The transverse dimensions of the child's head were distinctly shortened after delivery. The bi-parietal diameter was three inches and one eighth, whereas its average length is three inches and three quarters; the bi-temporal was three inches. The measurements in the antero-posterior plane were not diminished. The occipito-frontal measurement was four inches and three quarters, suboccipito-frontal, four inches and one eighth. Supposing the child to have come down, as it probably did, with the long diameter of its head in the transverse diameter of the pelvis, you will see that there was at least a quarter of an

inch in the pelvis to spare. This shows how large a margin there is in a normal state of things, and also to what extent the head of a well-developed child can be safely compressed. This child's head is not in the least behind the average in point of ossification; in fact, the anterior fontanelle is rather smaller than usual. It is usual in flattening of the pelvis for the child to enter the pelvis with the sagittal suture in the transverse diameter of the pelvis. As I have said, the child was only an ounce short of the average, and if we assume that the bi-parietal diameter was the average length, it was shortened by five eighths of an inch, showing the amount of compression the head will undergo without damage.

Supposing that in this case there had been difficulties, if the child had been larger, or if it had presented in an abnormal way, so that it did not enter the pelvis, my intention was to dilate the cervix uteri, and then deliver the patient by symphysiotomy. A child a good deal larger than this one could have been delivered safely by subcutaneous symphysiotomy.

I have drawn your attention to these two cases in order to impress upon you that the way to prevent difficult labour is, firstly, to make a point of examining the patient during pregnancy so as to early ascertain excessive size of the child with relation to the pelvis and induce labour if the child is of excessive size. Secondly, if the patient has gone the natural term of pregnancy before you see her, to find out early what the condition of things is, so that any treatment can be applied early. In the case of vesico-vaginal fistula, assuming, as appears to have been the case, the child was of excessive size, if the child had been living, in all probability by dilating the cervix with Champetier's bag and performing symphysiotomy, a living child could have been delivered. The risk is lessened if the procedure be carried out early. If symphysiotomy is done early, the risk is no more than that of natural labour. In Cæsarean section also success depends on the operation being done early.

There is another case, which I may mention here. It is that of a patient who had a large fibroid, discovered by accident four years before, which had been going on growing for the last six months. It reached to the level of the

lower ribs. The tumour was hard and solid and it was one with the uterus, and there was a large lump, one with the rest, bulging into the vagina on the right side. From these physical signs the diagnosis of uterine fibroid was made. Menstruation was excessive. The patient was a lady's maid, and she had been very properly advised by her doctor that this tumour was not likely to bring about her death, and therefore if she liked to put up with it she could. She said she could not get her living if this tumour persisted; it was so large and the hæmorrhage from it was so great that if she kept her tumour she must give up her situation. It also caused her a great deal of pain, and for these reasons she wished to have an operation performed. There was a special reason in this case for consideration before operating, which was that she suffered much from asthma and bronchitis. This made me apprehensive lest the anæsthetic might increase her bronchitis and asthma so that she might have severe fits of coughing after the operation, and that thus the abdominal wound might be burst open. This accident happens sometimes and it is not a desirable one. I asked for Dr. Stephen Mackenzie's opinion of this point, and Dr. Mackenzie reported that according to the patient's statement the asthma was worse in the summer, but he could find no signs in the patient's chest which called for postponement of the operation; he thought that if chloroform were administered bronchitis might not follow, and that possibly the removal of the tumour would improve the respiratory embarrassment. This is one of the points on account of which I shall ask your attention to this case. She had the operation done and the anæsthetic was chloroform. She had some bronchitis for a week or so afterwards, which for a day or two was severe, but the final result was that she says her asthma has been much better since the removal of the tumour. I wish to mention one point about the operation, and this is the assistance given by enucleation of the tumour. When the operation was done the round ligaments and the broad ligaments were tied as usual, and the left uterine artery was tied without difficulty. Then it was found that the lump, which had been felt by the vagina, was a large fibroid so bulging into and separating the layers of the broad ligament that it was not pos-

sible to grasp the uterine artery to tie it. I cut through the uterine wall over this tumour and reached its capsule, and shelled out the tumour from its bed with the hand. Then the folds of the broad ligament could be brought together, the right uterine artery could be easily felt and tied, and the body of the uterus was cut away without hæmorrhage. The tumour weighed six pounds and three quarters.

I have one other case to speak about, that of a patient aged thirty-eight. Her complaint was of pain in the abdomen and falling of the womb. The latter she had had for years. The pain in the lower abdomen was only recent. Her worst attack of pain was three weeks ago, and with that attack she felt faint. There was no alteration in her menstruation. She had a soft elastic swelling extending to the umbilicus. It was bulging in the left lateral and anterior fornices. The cervix was pushed over to the right, but there was nothing wrong with it. The case gave us some difficulty in diagnosis, because the tumour appeared to be one with the uterus, which was enlarged, the sound passing three inches and three quarters. By the rectum the swelling was found to be in front of the rectum, so it was not in the cellular tissue. Operation was performed and the tumour was found to be a dermoid cyst, which was adherent to the uterus, to the omentum, and to bowel. There was one unusual condition about this cyst, namely, that it was full of solid fat. Usually the fat in a dermoid cyst is fluid at the temperature of the body, and only becomes solid after withdrawal from the patient. In some of these tumours the fat is solid and made up into balls with whitish-brown fluid between them. How these pellets are formed is a mystery. It has been suggested that each of them has a hair for a nucleus, and that the fat clings round this hair. I know not whether every single pellet has been examined to find if this is so. But in this case the fat was not in pellets or balls; it was solid fat and I got it out of the cyst with a table-spoon. Similar cases have been recorded, but they are rare and I have not seen one before. There was a little yellowish-brown watery fluid as well as the fat, but not much of it. This dermoid appears to be a simple cyst; at least the cyst-wall has been examined by Dr. Andrews, and no other cysts have been found in it.

Ovarian dermoids have been divided into three classes: (1) Those which are simple cysts; (2) Those which have attached to them the ovary or ovarian tissue containing small cysts; and (3) Those which are part of multilocular tumours. It used to be thought that a dermoid was a different kind of cyst from the ordinary multilocular tumour, but opinion is coming more round to the view that there is not an essential difference between the different kinds of ovarian cysts. In dermoid cysts the skin-like tissue is not found over the whole surface of the cyst; often the main part of the cyst is like that of an ordinary multilocular cyst, the dermoid tissue being confined to a small part of it. The proportion of dermoids which are found to be single depends on the care and thoroughness with which the cyst-wall is examined. Besides fatty matter, these dermoid cysts contain hair and other skin appendages, such as sebaceous glands, often nails, and sometimes teeth; they contain irregular pieces of bone, but never any definitely formed bone. They are said also to contain breasts and nipples, but whether these breasts are rightly so-called is doubtful; they are nipple-like projections with gland tissue in them. Dermoids have been described in which regularly formed bones and limbs occur, but these were either merely resemblances to bones and limbs, or fœtuses from ectopic pregnancies. All the tissues formed in a dermoid cyst are produced by the epithelium of the ovary, and are such tissues as are produced by the external layer of the blastoderm, the epiblast. Structures produced by the mesoblast, such as muscles and bones, are never found in dermoid cysts. When a dermoid is ruptured, bits of it may be broken off and take root on the peritoneum, and in that way small dermoid tumours may be found growing all over the peritoneum. Sometimes malignant degeneration of dermoids is spoken of. Bland-Sutton has investigated this point, and has not been able to find a single case in which malignant recurrence took place after removal of a dermoid tumour. I suppose dermoid tumours do not bring with them protection against malignant disease, and therefore one would expect sometimes, as a coincidence, that dermoids and malignant disease would co-exist. But I take Bland-Sutton's research as sufficient to show that dermoids do not

tend to become malignant. When dermoids rupture the usual result is peritonitis. It is possible for a dermoid to rupture and for the fat it contains to escape into the *cœlom* without causing peritonitis. Nevertheless, peritonitis is the usual result, because dermoids are very prone to bacterial invasion, inflammation, and softening.

The explanation of this patient's attacks of pain was that the cyst was adherent, and when the adhesions had been separated and the cyst was free it was found that its pedicle was twisted in three half turns. The common effect of twisting of the pedicle is that the veins become blocked and not the arteries, and that the tumour becomes congested; hæmorrhage takes place into it, and the Fallopian tube beyond the turn is found thickened and of a deep purple colour. In this case it was not so. The pedicle was twisted, but it was not very vascular nor congested, and there was no hæmorrhage into the tumour.

One clinical peculiarity of dermoids is the slowness with which they grow. Slow growth indicates that a tumour is not very vascular. I take it that the small effect on this tumour of the rotation of its pedicle was because the pedicle was not very vascular. The vitality of the tumour was impaired by cutting off the blood-supply, and so it was beginning to get its blood-supply from omentum and bowel. Sometimes when adhesion has taken place in this way, the pedicle has been detached and the tumour has got its entire supply from elsewhere. I removed a cyst some time ago in which the pedicle was atrophied to a string of fibrous tissue, and the tumour was stuck on to the end of the opposite Fallopian tube and had got its blood-supply from that. This twisting was the cause of the attacks of pain. The usual symptoms of twisting of the pedicle are that the patient gets a sudden attack of pain, with pallor, and then, following that, symptoms of peritonitis.

The causes of the pedicle being twisted are numerous. The common ones are movements of the bowel imparting movement to the tumour; muscular effort on the part of the patient; accidents of child-birth.

The diagnosis of twisting of the pedicle can only be made, if it can be made at all, by the history. If a patient with an abdominal tumour gives

a history of sudden pain in the abdomen, with pallor following that pain, and fever, you may make the inference that there has been twisting of the pedicle of the tumour. There are two cases on record in which the diagnosis has been made by feeling per rectum the twists in the pedicle. But I think the cases in which that can be done must be uncommon. The patient about whom I have been speaking has had her cyst removed and is doing well.

A CLINICAL LECTURE

ON

ALBUMINURIA IN YOUNG MEN.

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WE have seen during the last few months many cases of albuminuria in this room, and there is nothing to call for remark in this fact. Of all chronic diseases in this country, next to those of the lungs, those of the kidney are by far the most frequently met with. Thus in out-patient practice renal disease is, next to phthisis and chronic bronchitis, perhaps the malady most commonly observed. This being so and the subject of renal disease being excessively trite, you may perhaps be surprised at my selecting albuminuria as a symptom for discussion this afternoon, but I hope to show you directly that albuminuria is often not a symptom of renal disease and that it has frequently little or no import, at least none that we can exactly determine and define. You know that it is my habit in all cases, when the patient is a male, to make an examination of the urine, and when this is done it is surprising how often albumin is found in the urine when its presence is totally unsuspected. I dismiss all cases in patients over thirty and in middle-aged people generally in which albumen is detected, for in such cases it may be safely attributed to the presence of chronic renal disease. Not so, however, in another very large class, examples of which have been very frequently met with of late in this room, in which the patient is young, often between fifteen and twenty, and never over

twenty-four or twenty-five. These youths present themselves perhaps for some trivial affection—a cough, a cold in the head, a pain in the abdomen, and were it not the custom to get them to pass water and to submit the urine to the usual tests it would never for a moment be supposed that there was anything morbid to be made out concerning the urine. Strange to say, however, in a very large percentage of male cases albuminuria is present, and often in very notable proportions, the origin of which is by no means easy of explanation. I need scarcely allude to the fact that this subject has assumed a considerable importance, indeed a very great importance if the quantity of literature concerning it be regarded as a measure of the gravity of the affection. And I need scarcely say that of theories on the causation of this kind of albuminuria we have enough and more than enough. Indeed, so theory-laden is the subject that it is becoming somewhat repellent, merely on this account. It is becoming also obscure owing to the want of precision and of definition in the terminology employed. Among the variety of terms applied to this form of albuminuria the following are examples:—"albuminuria of adolescence," "albuminuria of position," "functional albuminuria," "cyclical albuminuria," "food albuminuria," etc., etc., the result being confusion worse confounded. The ineradicable tendency of the medical mind to theorise, to jump to conclusions, to form hypotheses, is nowhere more conspicuous than in this matter of the albuminuria of young men. That it is due to standing, to over-exertion, to certain kinds of food, and to numerous other causes is maintained by different observers, while others are content with describing the affection with reference to the time it appears, to the position of the patient, and so on. All this is very unsatisfactory, and what is really required is some definite guidance as to the meaning of this form of albuminuria. Directly the inquirer seeks for this guidance he is at once thrown into confusion. He will find that what one will call dietetic albuminuria another will describe as cyclical; what in some books is defined as functional albuminuria will, by another "authority," be put down to "cold." And so on. Still further is the difference of opinion as to the meaning and prognosis of such cases enormous. By some it is looked

upon as trivial and of no importance, either in diagnosis or prognosis, by others as being evidence of structural renal disease, and no less; a third type of commentator will withhold his opinion altogether. Where shall wisdom be found? Surely not in books, special works or others, and clearly not from the comparison of opinions which are diametrically opposed. It seems to me that the best way of forming an opinion is to observe for yourselves, to see the class of case to which I have called attention, to examine the individuals comprising this class, to follow them up as far as possible in the outpatient room, and to observe the effect of local treatment. At the same time it is advisable to avoid reading any "authority" on the subject, in other words to keep the mind thoroughly unbiassed. We have now for many months followed not a few of these cases and I will recapitulate the results obtained, at the same time pointing out the features of different classes of the affection. And first of all I should like to point out two aspects of the subject which are apt to cause confusion. Albumin in the urine may be met with, the fluid being perfectly clear with the exception that certain small, more or less roundish, shreddy particles float in it. The clearness of the fluid shows at once that it is not a purulent urine, using the term in its ordinary sense, and the shreds to which I have alluded are so small that they may escape detection, or be attributed to foreign particles contained in the receiving glass. In such cases the microscopic examination of the shreds will at once prove that they consist of agglomerations of pus-corpuscles. Now the patient, if interrogated, will admit that some months or a year ago he had gonorrhœa, but that for a long time the disease has absolutely disappeared. The fact is that somewhere in the course of the urethra there is a villous patch on the mucous membrane which is the seat of a chronic discharge, and to the presence of this lesion the albumin in the urine is due. I need scarcely say that with the treatment of these cases we have nothing to do; they are surgical cases, pure and simple. Nevertheless, though such conditions are not common, yet they do occur, and it is necessary to be aware of the fact in order to avoid mistakes. The other danger is that of mistaking for the form of albu-

min which I now discuss that which is really the result of bad habits. The microscope here again will settle the question, for in cases of this description spermatozoa are sure to be encountered. It is very rare, in my experience, to meet with albuminuria owning this origin. Having now cleared the ground I may say that, roughly speaking, albuminuria may, in the case of youths and young men, occur in those who are in every respect in good health. This class, which contains by far the larger number of cases, is distinguished by the entire absence of any signs or symptoms of renal or other disease; and, unless the urine be intentionally examined for the express purpose of ascertaining the presence or absence of albumin, it would certainly not occur to any observer that such an abnormality as that really present could exist. The second group contains those who are to a certain extent flabby and anæmic, this condition being generally due to their vocation, but otherwise they are in good health and quite free from evidence of the presence of any organic disease. The third group includes those cases in which again disease of some kind is present, but is not that affecting the kidney. For instance, valvular heart disease may be detected, but, as the compensation is perfect, it is not rational to attribute the albuminuria to the stasis due to failing heart. It is noticeable that in all these classes, but more especially in the first and second, there is a great tendency to the presence of oxaluria. Crystals of oxalate of lime are constantly found and the association of the two conditions is so marked that a closer connection between them than that due to accident would seem to be implied. Having already shown the absurdity of the theoretical position of the malady, it is necessary for me to say that I am not in the least disposed to advocate any theory whatever as regards the cause of it, but I cannot refrain from pointing out that albuminuria will be induced in perfectly healthy and robust individuals by exposure to cold. The form of exposure which I know to be effective is that experienced when open sea bathing is practised—in other words bathing from a boat. If, in the coming summer, you will take a boat, carrying with you a bottle of nitric acid and a few test-tubes, you can prove the correctness of this observation.

All you have to do is to test the urine before entering the sea and again immediately on returning to the boat. The previously healthy urine will show a marked cloud at the junction of the nitric acid. The only difficulty is in adjusting the delicate movements required in pouring the urine into the acid; this, owing to the oscillation, is not always easy. You will find that albumin will be detectable for some twenty minutes or half an hour after coming out of the water, but in every case will cease soon after the surface becomes glowing and when the body is again covered. These facts, as I have already said, can be easily demonstrated, and as I have myself observed them I am not speaking at second hand. It seems to me that this effect of exposure to cold is a very significant one. You will have noticed that in many of the boys and young men who have been affected with this form of albuminuria there has been the habit of open air bathing. So frequently has this been the case that in every case I specially inquire whether recent and frequent bathing has been indulged in. You know very well the tendency of boys in warm weather to remain in and out of the bath; they cannot be induced to take a plunge, dry themselves and dress, but will insist upon prolonging the exposure to an inordinate length. In other words, we meet with the precise condition which draws the blood from the surface and therefore tends to gorge the viscera. Now it has been maintained that in many cases of this form of albuminuria the condition is due to the maintenance of the erect posture. It is said that this is proved by the fact that the appearance of albumin in the urine ceases as soon as the patient assumes the recumbent position, or, in other words, is put to bed. You will, I daresay, recall the case of a young man of twenty whom you have seen very often in this room, and in whose case the presence of albuminuria was both constant and marked. You may remember that this patient was admitted to the wards and kept strictly in bed, the result being that all trace of albumin disappeared at once. Does such a case as this tend to prove the correctness of the theory of position influence on the malady? I do not think so, for this very cogent reason. When a patient is kept in bed he is naturally kept warm; his skin is comfortably hot and he

is under the blankets kept at a uniform and constant temperature. In other words he is entirely free from the effects of chill applied to the surface, effects which we have seen to have a potent influence in causing albuminuria. You cannot dissociate the effects due to posture and to chill when the patient is lying in bed, and I am as much entitled to attribute the disappearance of the albumin to warmth as are the supporters of the posture theory to their explanation of the phenomenon. It is further to be noticed in this case that, on several occasions, when he was up and about, the patient's urine still remained albumin-free, he being well wrapped up, all danger of chill being avoided and the weather being fine and warm. May not then some forms of this abnormality be due to internal stasis, itself the result of chilling of the skin; and, at all events, is not this view of the case quite as reasonable as that which attributes the affection to posture. I am perfectly aware that the occurrence of albumin in healthy urine after cold bathing is no new discovery. Years ago the fact was known, but of late it seems to me to have been lost sight of. In fact, so much time and attention has been devoted to subdividing and splitting up the subject that a rational view of the whole matter seems to be in danger of being neglected. Now this subject is, I am convinced, one and indivisible. Cyclical albuminuria is nothing but another name for this identical affection. Very often indeed, in the cases I hear of, the albumin is present at one time, absent at another. Over and over again you have seen this demonstrated here. A patient seen on the Monday with a large quantity of albumin in his urine presents himself on Thursday and the urine is found to be free of all abnormality. A striking instance of this kind some of you saw some months ago. The patient was an example of the cases relegated to the third class. He had mitral stenosis, but as regards the symptoms of heart failure, he was perfectly free from any such, the compensation being sound. His urine contained about a third albumin; he came a week later, and not by the most careful testing could evidence of albuminuria be obtained. The albumin had entirely vanished. The case was of great interest, inasmuch as at the first examination not only was albumin seen but casts also were

present, and in large numbers. This was a very remarkable fact, but the discussion of this aspect of the case I must reserve for another lecture, as it would lead me too far away from the matter in hand—the significance and causation of the special form of albuminuria to which I have drawn attention. Why albumin should appear *more readily* in the urine in some cases than in others where the same exciting cause is present can only be explained on the same principle as the well known tendency of some people to catarrh on the least exposure. No doubt the susceptibility varies enormously in different subjects when the question of chilling of the surface is considered. I lay stress on the words *more readily*, for it seems to me that if the exposure is sufficient we should all, as the result of that exposure, become albuminuric. It is only a question of degree, not one of kind. And it becomes us not to dogmatise on the abnormality of the presence of albumin in the urine, when it is considered that, even at this time of day, we have not in any sense thoroughly studied the condition of the urine in apparent health. This is a most important matter, and one that receives little or no attention. Abnormalities are looked for on all sides, but the condition of the organism in its supposedly normal relationships is neglected. In this respect it seems to me that the history of auscultation is repeating itself. In the early days of the stethoscope every patient with a murmur was regarded as the victim of deadly disease, and only of recent years has it been ascertained that great numbers of perfectly healthy people carry murmurs (systolic only, it is true) through months or years without detriment to their health. Just in the same way increased investigation of the urine tends to show that conditions formerly regarded as most serious may really have little or no significance. Are we then to say, off hand, that albuminuria without casts in a young man is a matter of no importance? By no means. In this, as in all other cases, a diagnosis can only be arrived at after a thorough and detailed examination of all the system and organs, together with an adequate consideration of the history of the case. But I do say that when the examination shows entire absence of the renal heart, by which I mean that neither hypertrophy of the left ventricle, reduplication of the first or accen-

tuation of the entire second sound are present, a pulse of normal tension, and no œdema or dyspnoea, then it is in the highest degree probable that the kidneys are perfectly sound, and be the cause what it may, no anxiety need be felt on account of the presence of albuminuria. Of course in such a case as this it is understood that casts are entirely absent. The form in which this variety of albumin often presents itself practically relates to the question of life insurance. A foolish young man wishes to make an impecunious marriage and is face to face with the necessity of insuring his life—the only means open to him of making a provision for the future. Probably the case appears in all respects a most eligible one, but, greatly to the surprise of the observer, when the urine is examined it is found very albuminous, though not persistently so. In such cases a heavy responsibility rests on the medical adviser to the office—he must secure the latter from undue risk while at the same time he has every wish to protect the applicant. You will not find much help in the practical management of such a case in the literature of the subject, but from what you have seen in this room I think you may formulate certain rules of procedure. We have now seen, off and on, for many months some of these cases, which I have purposely kept under observation, and, although it is true that in many the albumin has not disappeared from the urine, yet in some this has been the case. However this may be, one thing is certain: that in practically every case the health has not deteriorated in any way, and in not a single instance has the heart shown the changes incidental to chronic renal disease, and the pulse has remained soft. This is particularly noticeable in the case of the young man who spent some weeks in the ward last summer, and as he was passing considerable quantities of albumin, and this more or less continuously except when in bed, it may be regarded as a test case in this matter. If then you have to decide the fate of such an applicant it is in the first place necessary to postpone the final verdict, and during this time, say three months, the candidate should be kept under skilled supervision. In this way repeated examination of the urine and of the vascular system, and a consideration of the state of the general health, will enable an accurate

judgment to be made as to the risk, if any, of the case being a hazardous one to accept. If, at the end of three months, all evidences of renal disease are absent and the general health remains good, it seems to me that the case might be taken at a slightly increased rate. And for this reason: We have seen how closely connected these cases are with chilling of the cutaneous surface, and hence it is quite conceivable that if the patient, through carelessness or in consequence of his avocation, is likely to be repeatedly subjected to this chilling process, what is at first merely an ordinary stasis in the kidney might, by constant repetition, become a renal inflammation. It is to guard against this risk, not perhaps a serious one, that I think some addition to the premium should be made. And now what is to be said as to the management of these cases, apart from any question of life insurance. Nothing but common sense is necessary in this respect. Let the subject of this singular affection avoid all undue exposure both in summer and winter, more especially cold bathing in the open. And let him at all seasons of the year be warmly clad. Nothing further is required in my judgment, and to treat such persons as if they were really suffering from chronic renal disease and to fix their attention on the kidney is the surest way to make them miserable valetudinarians, if not actual hypochondriacs. You will notice that I have said nothing about drugs. They are totally unnecessary in this affection and are far more likely to do harm than good. I have made no reference to the state of the circulation in the Malpighian tufts, nor to pressure changes in the same. It seems to me to be a pure waste of valuable time to criticise theories which are based on changes of which we know nothing, and as we are absolutely ignorant of the matter assumed by the elaborators of such theories, it is perfectly futile to pay the least attention to their effusions. It seems to me that a practical acquaintance with this form of albuminuria may be of some slight use in actual daily work, whereas nothing is more certain than that a perusal of the literature of the subject will arouse a feeling of disquietude, of vacillation, and of distrust. You know the quotation from Goëthe:

“Grau, theurer Freund, ist alle Theorie
Und grün des Lebens goldner Baum.”

And the sentiment is particularly applicable to medical theory, which is above measure grey and barren. If you require a *working hypothesis* you have it in the relationship between surface chill and albuminuria in healthy people. It is perfectly open to anyone to proceed by analysis from this fact; but the whole chaos of crude theory which has been elaborated concerning the causation of this affection should be relegated to that oblivion which, sooner or later, is its inevitable destiny.

THE RESPIRATORY AFTER-EFFECTS FOLLOWING THE INHALATION OF ETHER.

A discussion at a Meeting of the Society of
Anæsthetists.

The President, Dr. J. FREDK. W. SILK, in the
Chair.

DR. DUDLEY BUXTON said:—Mr. President, Ladies, and Gentlemen, I propose on the present occasion to attempt to go over the ground which, during the last few years, has been covered by various experimental and clinical observers, who have considered this most important subject of the after-effects following ether; and to incorporate, very shortly, my own experience. There are probably three most important questions involved. The first one is whether one can attribute any respiratory after-effects to ether pure and simple; the second is, what is the pathogenesis of these complications, if they exist; and the third is, how are such complications to be prevented.

I would, in the first place, express my regret that a great deal of looseness has crept in in the descriptions which one frequently hears among one's surgical *confrères*, about "ether pneumonia," "ether bronchitis," and what not. Now, these names have been, unfortunately, badly chosen, because there is no reason to believe that

"ether pneumonia" is itself an entity or a disease *sui generis*, and the mere fact of using these terms for pneumonia, etc., following the inhalation of anæsthetics predicates a pathogenesis to which I, for one, should not subscribe. I have found it extremely difficult, in consulting English literature on the subject, to find any very definite statements, or, indeed, more than a few definite descriptions of cases of such complications. In nearly all of these there have been so many other probable influences at work, that one has been almost compelled—I was going to say, against one's will, but certainly against one's better judgment—to attribute very many of the cases which have been reputed to be true cases of respiratory trouble following ether and due to ether, to causes other than the action of ether. At all events, in such, the causes at work, which, while they may have involved ether, certainly included other factors beside the ether. And the same confusion has obtained in the continental journals which I have had time to consult. I may say, in passing, that an enormous amount of valuable experimental work has been done abroad, especially in Germany, but the clinical material which one has been able to tap has been highly unsatisfactory in the following respect:—Although certain cases have been noted, and in some instances carefully followed, yet what, to me, is the most important factor in the whole problem has been omitted. I refer to the method in which the ether has been given. It would be obviously an absurd thing to attempt to prove that some usually harmless material such as, we will say, butter is highly detrimental to health, unless one knew the amount of butter which an individual who died from consuming it had taken. Probably when given dosimetrically, butter would be wholesome and palatable, but, if given in much larger quantities, it would be prejudicial to the health and the comfort of the individual. So I take it, in the case of ether, very many cases of ether complications so-called have arisen, one may presume, from the method one has seen practised abroad, and that the trouble is due to the faulty method rather than to the anæsthetic. But at the same time we must recognise that, as the strength of a chain is tested by that of its weakest link, so we must be prepared to either admit or deny that ether does, whether well or badly given, give rise

to pulmonary complications. Therefore I do not propose to press this matter further in my present remarks, but to refer to it later, when I attempt to show that there are means by which complications such as do arise may, by care and skill, be prevented.

Then I should like to make another prefatory remark, and that is with regard to chloroform. Our discussion is confined to ether, and perhaps, in some ways, it is a pity we are tied down to that. I am sure our President will permit me to wander a moment from the beaten track of duty and refer to chloroform in this connection. What I want to say is, that you rarely hear of chloroform bronchitis, or chloroform pneumonia, but if you take the trouble to refer to your own records, or to the records which have been published, you will find quite a large number of cases of pulmonary and respiratory difficulty following the inhalation of chloroform even when it has not been given by candle-light. I purposely exclude cases of poisoning by burning chloroform, as they belong to another category. I believe the reason that surgeons exclude from their minds the idea of chloroform bronchitis is this:—That if a patient after inhaling chloroform has bronchitis or broncho-pneumonia, he or she is said to have taken a chill, and the pulmonary symptoms are put down to that chill, whereas when a patient is inhaling ether, in a very few moments you hear loud râles in the chest in certain cases, which to the surgeon at once appears to be due to an alarmingly acute, almost fulminating, attack of bronchitis, and there is little doubt that if you ask him before he has time to think out the question he will say at once that the patient is suffering from a respiratory attack due to the ether. I believe, then, that it is a fact that chloroform bronchitis is not recognised as a regular and necessary sequela to the administration of chloroform, whereas if there is evidence of something wrong in the chest following the administration of ether the average surgeon would diagnose ether bronchitis, while he would deny or pooh-pooh the possibility of chloroform bronchitis.

I may say, referring to an attempt which I have made to arrive at the frequency of the occurrence of these respiratory complications of ether, that I have found myself plunged into

great difficulties. No accurate statistics have been kept, as far as I can gather, at all events, if they have they have not come into my hands. Probably in the case of the London hospitals they have been kept, but the occurrence of pulmonary complications has happened in so extremely few instances that they have hardly counted. But various gentlemen, especially in America, have collected a certain number of cases, and I shall refer to these; and I shall also refer to an account I have received from various practitioners and surgeons giving a rough idea of the frequency of pulmonary complications following the use of ether. Then, in a very valuable paper by Dr. Schultze we find a record of seventeen cases out of 4,914 administrations. But of these there were seven cases in which pneumonia commenced on the seventh, twenty-fifth, fifth, fifth, fourteenth, tenth, and fourteenth days after the inhalation of ether; of two other cases one occurred after four days; only two occurred within twenty-four hours of the operation. The question of the time of onset of the respiratory complication after the inhalation is one of the greatest importance, and I shall hope to revert to that at a later period. I may say that I am fully aware that some authorities, men of considerable light and leading, Lienau, Unger, and Fränkel, have distinctly stated that they regard twenty-five days as the period within which pneumonia may occur as the direct result of ether. I cannot enter into the matter at all fully, as it would take far too long. The experiments of Unger and Fränkel did not appeal to me as having substantiated the position which they had taken up. The reason why I should distinctly doubt that the cases I have mentioned were true cases of "ether pneumonia" is simply because there are so many other factors at work after say twenty-four hours, in the average hospital ward, which certainly cannot be eliminated in the hospital cases; and I take it that for scientific proof we must be able to fairly eliminate many or all causes beside ether in arriving at any accurate result upon the question of the ætiology of the so-called ether pneumonia, ether bronchitis, and so on. Of course one is bound to admit that in private practice and in surgical homes these observations do not apply with the same force. It is quite conceivable

that a patient may be kept free from draughts and exposure in these classes of patients; but, curiously enough, I have found, from those who have been good enough to afford me information, that it is extremely seldom, if ever, that they find cases of pulmonary complication occurring in private practice. The cases that they have met with and recorded have been, in almost all instances, those of patients whom they have treated in hospitals. Julliard tells of 4,000 cases without any lung complication—I am quoting from his pamphlet which was published a good many years ago, and I understand from him that he is still in the position of never having met with a case of so-called ether pneumonia or any lung complication. Mr. Mayo Robson, who has kindly placed in my hands a very valuable letter containing his experience, has never met with such complications in private practice, but he has met with them in hospital. Then I find Dr. Hewitt in his book mentions having met with two cases in ten years in which bronchial irritation followed the use of ether. What the bronchial irritation exactly was does not transpire, and I imagine it was a mild form of bronchitis or broncho-pneumonia. In both of these cases cough, and in one bronchitis were present before the inhalation. Then again, in the report of the London Hospital for 1897, the same observer records that out of 2,910 ether cases there were one of broncho-pneumonia, and one of hypersecretion of mucus leading to asphyxial conditions. If I am wrong I appeal to Dr. Probyn Williams, who assisted Dr. Hewitt in drawing up the report, for I find it a little difficult to arrive at the meanings of the figures in the report. In the case of hypersecretion which is mentioned it appears that the patient was about to vomit, that the teeth became clenched and he became deeply cyanosed, and the house surgeon at once performed tracheotomy. Mr. McCardie has also published one case of lobar pneumonia, and one case of broncho-pneumonia in 310 etherisations; and in both of these he assures me no other cause than the inhalation of ether could have existed. He has also furnished me with particulars of other cases. They are very briefly put, and I should like to place them before you, as they will perhaps help us in our after-discussion. One was the case of a

girl who suffered from extreme shock. The operation took twenty minutes; it was an amputation. Bronchitis was followed by broncho-pneumonia. The date of the onset is not given. In the second case a woman in ordinary good health appears to have been given very little ether, and the operation was amputation of the breast, which occupied an hour. She had lobar pneumonia which developed on the fifth day. In the third case the inhalation was commenced with chloroform for ten minutes, and then ether was given for the greater part of an hour. Oöphorectomy was the operation. The patient had a little hacking cough apparently before the inhalation, and double broncho-pneumonia occurred afterwards. This came on immediately and was of a very severe type. In the fourth case the operation was laparotomy, and broncho-pneumonia gradually developed after the operation. The author speaks of one other doubtful case. My own judgment would lead me to exclude a case which came on five days after the operation but for the fact that McCardie states that he is sure that there was no undue exposure of the patient—though what constitutes undue exposure in any given case I do not know. It appears to me that in a large proportion of cases there is undue exposure from the point of view of the possibility of catching cold, even if not from the surgical point of view. Then there are cases published by Professor Drummond, who gives figures which are extremely involved. I believe he recorded eight cases and three fatalities. Then again in the records of Gurlt one finds mere bare statements which amount to this: That most of the deaths due to ether were the results of ether pneumonia. And when one is aware of the methods of giving ether in vogue in Germany, one is not surprised at ether pneumonia occurring pretty generally in German clinics. Witney also speaks vaguely of many cases. Anders, whose paper I have not been able to consult at first hand, but who is quoted by McCardie, gives forty-six cases of pneumonia in 57,842 cases of etherisation, which gives a proportion of about one in 1,250. It appears he himself collected thirty cases out of 12,842, or one in 423. About these cases there are no particulars, so one cannot attach very great importance to them. In my own experience I am bound to admit that lung complica-

tions are extremely rare. In spite of Anders' statistics of one in 300 or 400 cases I should regard lobar pneumonia as one of the rarest of complications following ether, while bronchitis and broncho-pneumonia, although more commonly met with, are yet quite rare. In private practice I do not learn of any cases of pneumonia which could be attributed entirely, or even in part, to ether, and with hospital patients I should say the most common complication was bronchial catarrh, commencing within twenty-four hours of the inhalation and disappearing in two or three days. In one case of pleurisy and pleuro-pneumonia which followed the use of ether, the operation was for liver abscess, and I have very little doubt, and I think the surgeon had very little doubt, that other causes were at work besides ether in bringing about these undesirable complications. In several hospital patients, whose trouble has been loosely described to me as pneumonia, I have found on examination bronchitis of a mild type, without the typical physical signs or temperature of pneumonia. Probably many of the so-called pneumonias from ether are explicable by similar reasoning. As to the type of the disease, I have failed to discover from the few cases which have been sufficiently studied and published, such as the meritorious work of Dr. Drummond, that any of these conditions possess a type of their own. As I believe I mentioned at the commencement, the pneumonia is a common pneumonia, the bronchitis is a common bronchitis, and the broncho-pneumonia is a common broncho-pneumonia. I say that with this reservation: One does find that the course some of these complications run is changed by the condition of the patient and by the exigencies of the surgical procedure. One finds, for example, in ordinary cases, and in cases where the radical extirpation of mammary cancer has been attempted, that the tight bandaging up after the operation has distinctly assisted, or one believes it has assisted, in bringing about the lung trouble, and has seriously interfered with the prognosis of the case. And one has been forced to admit that there are cases, as shown by Dr. Drummond, of very slight bronchial irritation which have, owing to the disabilities of the breathing organs caused by tight bandaging, gone on and run an absolutely bad course owing to the extreme diffi-

culty which the patient has experienced in coughing the secretion from his lungs.

I should like to give you the experience of one or two anæsthetists. Dr. Cock tells me that in a ten years practice he has never seen a case of pneumonia following the administration of ether, and he has seen only one case of bronchitis following it. This case he believes was due to the exposure to cold, owing to the evaporation of antiseptic fluids as used in the old form of operating. Then, referring to the causation of these troubles, we have first to consider the ether, then the exposure, and perhaps other factors may occur at a later stage. With regard to the ether, you are fully aware, I have no doubt, that a great deal of experimental work has been conducted with the attempt to show that the ether itself may give rise to pneumonia in one of two ways. The extremely important work of Hölscher, which has opened a new era in the discussion, carries us to this point, as shown by his experiments, that under ordinary conditions the patient aspirates the mucus and saliva of the mouth into the air-passages. There is usually hypersecretion of mucus due to an over-irritation set up by the ether vapour. The aspiration into the lungs causes, according to Hölscher, the loud so-called râles, really false râles, which are heard at first in the trachea and later on in the lungs as the fluid passes from the trachea into the larger bronchi. Hölscher's view, which has been elaborated by Whitney, is that the mouth contains a large number of pathogenic micro-organisms and that they pass with the mucus into the lungs and there give rise to pneumonia by direct infection by the pneumococcus. Lindemann, who has undertaken the most careful research on the subject, points out that if the views of Hölscher are to be accepted in their entirety, it is most remarkable that more cases of ether pneumonia do not occur, because aspiration, at all events in the hands of inexperienced etherists, must commonly occur, and the result ought to be the occurrence of a true infection. And that takes me to another point for a moment, one which explains, I think, the criticism of Lindemann. It is this: The condition of the patient who aspirates fluid in his lungs is not that of the normal individual. It is curious that that fact has been overlooked by all those who have worked upon

the subject; that under any anæsthetic the bodily temperature drops between 2.5° and 4.4° in a prolonged operation. This fall of temperature, of course, has but one meaning; that the vitality of the individual, in other words his resistive power, is very much lowered, and this must vary according to the length of the operation, its gravity, and the physical condition of the patient at the start. And probably it explains why certain persons succumb and other patients do not. Then the other view adopted by various observers whose names I need not mention, is that ether acts entirely as a local irritant. It is aspirated into the lungs, with the mucus which is present in the mouth, is worked into the bronchi, and there drops by its own weight into the fine cells. The experiments which have been undertaken show that in prolonged etherisation, where a considerable quantity of ether is given—a point we must revert to in a moment—you get the ultimate air-cells filled with blood-clot and altered epithelium. Those results only obtain when a very large quantity of ether is given. In ordinary cases, a mere rubescence is found on the cells, which rapidly passes off, no permanent desquamation of the epithelium taking place.

After this very hasty glance at the probable pathogenesis, from the experimental point of view, of these cases of respiratory trouble following ether, one may revert to the clinical side. Now, what obtains under ordinary circumstances? The patient, who, in very many cases, is depressed by the disease which induces him to seek surgical help, is taken into a hot operating theatre, here he is stripped, or the major part of him is uncovered, and then his temperature is lowered by a prolonged administration of ether, in very many cases more ether being given than is at all necessary for purposes of anæsthesia. He is then moved along a draughty corridor into a ward where two or more windows are always open. That, I think, is the true pathogenesis of most of the hospital cases of pleuro-pneumonia, bronchitis, and so-called "pneumonia." In other cases, I am quite sure that initial lesions are overlooked. Very hasty examination of the patient is made, and as a matter of fact the lungs are hardly examined at all before the patient is given the anæsthetic. If he has a violent cough he escapes, because he is given another form of

anæsthetic. I will revert to the question of how to prevent the occurrence of pulmonary complications. That is really involved in what I have just said: the limitation of the amount of ether. When the patient is fully under its influence he should be given extremely little, at all times preventing cyanosis; because there is no doubt, as experiments have shown, that cyanosis produces stasis in the capillaries, and that the stasis certainly tends to pneumonia. Avoid all cyanosis, and have your patient wrapped up during the operation, and protected from draughts afterwards. I think when all these points are duly attended to, even the infrequent cases of pulmonary complications which have been recorded as following ether, will disappear. I may say that one reason for stating this is that Dr. Schultze, who published a large number of cases, says that in late years, in the Presbyterian Hospital of the City of New York, the cases have been getting year by year less frequent, and he says, "that, I believe, is due to the fact that those who give the anæsthetic are being more carefully taught, and the apparatus employed is more carefully attended to."

And yet one other point, and that is the question of infection through the ether face-piece. That ghost has already been laid. It has been shown that ether itself is a powerful germicide, and that the micro-organisms hanging about the face-piece are certainly injured, if not rendered *hors de combat*, by the ether which is inhaled. But I may mention an interesting research made by my friend Dr. Cock, who examined a face-piece at Guy's Hospital, which he carefully sterilised and then drew cultures from and failed to get any micro-organisms, and therefore concluded that the face-piece was sterile. He then gave gas and ether to a patient and again examined the face-piece. He then found it to be swarming with bacteria. That is just what one would have expected, and probably everybody who uses ether carefully cleanses his face-piece, so that the risk of infection from a face-piece is extremely small; and one hopes that those who do not use clean face-pieces will not have an opportunity of giving ether at all largely.

(To be continued.)

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A CLINICAL LECTURE

ON

RHEUMATISM IN CHILDREN.

Delivered at the Hospital for Sick Children, Great Ormond Street, London, October 5th, 1899.

By DAVID B. LEES, M.A., B.Sc., M.D., F.R.C.P.,

Senior Physician to the Hospital, and Physician to St. Mary's Hospital; Examiner in Medicine for the University of Cambridge and the Victoria University.

LADIES AND GENTLEMEN,—The change in our conceptions of the true nature of the disease which we call rheumatic fever—a revolution which is now going on, and which is bringing quite new ideas of its etiology,—is largely due to the study of rheumatism in hospitals for children; and, indeed, this hospital may fairly claim to have contributed much to that alteration of view, for I might remind you that it was Dr. Barlow who was the first English observer to appreciate the meaning and the importance of the subcutaneous fibrous nodules which are so striking a feature of rheumatism in many children. It was Dr. Cheadle who, in his "Harveian Lectures" in 1888, drew attention to the varied manifestations of rheumatism in childhood, and to the way in which one set of symptoms may give place to another of quite different kind, yet all equally rheumatic. And it was the late Dr. Sturges who, in his "Lumleian Lectures," laid great stress on the damage done to the heart by rheumatism in childhood, and on the considerable mortality in early life from this disease. So that this hospital has had a considerable share in the elucidation of the symptoms of rheumatism in childhood. For, indeed, if one thinks of the current ideas of rheumatism, one sees that the problem has been attacked at the wrong end. We have had descriptions of rheumatism as it occurs in adults, and the disease has been described as an arthritis, with occasional "cardiac complications," as they were called, but primarily and essentially an arthritis. It

has been so described because in adults the arthritis is, as we all know, the most obvious and the most painful rheumatic affection, and one that calls for prompt treatment. But I would remind you that in disease the most obtrusive symptom is not always the most important symptom. For an instance of this, take the question of cough in phthisis. On the other hand, you will remember that the facts of disease which are really the most important are often not at all obtrusive. If you want an instance of that, you have only to think how influenza produces an acute cardiac dilatation, giving little sign, perhaps, only the patient suddenly dies; and how in diphtheritic paralysis there may be various obvious paralyses, but what is of real importance is the condition of the heart. And here, again, sudden death may end the case. Similarly the arthritis in the rheumatism of adults, though the most striking part of the disease, is not the most important part of it. It is the arthritis which seems to the patient the most urgent matter, but it is the condition of the heart that determines the prognosis.

There was this disadvantage also about the study of rheumatism in adults only, especially as a question mainly of a variety of arthritis, that it was mixed up with various other forms of arthritis. It has been confused with gout; it is still often confused with gonorrhoeal arthritis; it is mixed up with suppurative arthritis, and also with chronic rheumatoid arthritis. So that in looking upon rheumatism as a variety of arthritis, there are these various difficulties of diagnosis which have to be considered. Also, from the theoretical point of view, the way of looking upon rheumatism as an arthritis makes it possible to explain it on a neurotic theory as a reflex from an external chill, or, on the other hand, to accept a theory of perverted metabolism on the lines of the true explanation of the phenomena of gout. So that rheumatism has been explained as a poisoning by some product of perverted metabolism, especially lactic acid, in the same way that gout is a poisoning by uric acid. And, indeed, there have been found those who maintain that rheumatism is a product of uric acid, just as gout is, though they do not very clearly explain why one is rheumatism and the other is gout.

But when we come to consider the disease in children, all these things disappear at once, for here arthritis is a very minor symptom, and in fact

it may be entirely absent. If present, it is usually quite insignificant, with but little swelling, but little pain, but little tenderness,—a fugitive inflammation affecting usually one or two joints, often only one, although, of course, cases occur occasionally in which several joints are involved. Practically, one may say that when one comes to study rheumatism in children instead of in adults, the arthritis, which is the dominant feature in adults, disappears, and so the confusions with gout and with other forms of arthritis disappear also. For though arthritis is frequent in childhood, and is of many varieties, yet there is usually little difficulty in diagnosing the arthritis of rheumatism because of its other associations. So that the various theories with relation to gout and conditions of metabolism which are tempting as theories when dealing with rheumatism in adults, are evidently insufficient when you come to explain the manifold manifestations of the disease in childhood. And as arthritis withdraws into the background of one's picture of rheumatism, certain other manifestations come into the foreground, and among them chiefly those connected with the heart. These, indeed, are of prime importance. The arthritis is simply a question of endurance of pain for a few days, but the affection of the heart is not only a temporary danger, it is also fruitful in results which remain permanently, and often curtail the patient's life by many years. Children may be quickly killed by a rheumatic attack. This statement will surprise you if you have not paid special attention to the disease as it occurs in early life, for the mortality of rheumatism in adults is small. It is not often that you see a fatal case of rheumatism in a general hospital in the adult wards. Occasionally such patients may die from hyperpyrexia if they are not promptly treated, and sometimes they die because they have already a severe form of heart disease, in which compensation breaks down under a fresh rheumatic attack. But putting these cases together, they are not very frequent, and most of the cases in adults recover, though often with more or less damaged hearts. But in children the matter is very different. I would refer you in the first place for a proof of the large direct mortality which may occur to a remarkable statement of Dr. Sturges in the Lumleian Lectures to which I have already referred. In speaking of the severe forms of carditis, chiefly under the heading of pericarditis, he mentions that of sixteen cases of

this severe type which he remembered, no fewer than twelve died. Now, if you think of what that means, you will see what an enormous effect upon the heart rheumatism has in childhood as compared with what occurs in later life. And for another proof I would refer you to the collection of 150 cases of fatal rheumatism in children under twelve taken from the records of this hospital, and in part from those of St. Mary's Hospital, by my friend Dr. Poynton. Of 115 cases in which the point could be investigated, it was fairly certain that thirty-five, or one-third, were *first* attacks of rheumatism. I have had a child under my own care who had been ill only a week when he died from his first rheumatic attack. He came into the hospital with pericarditis, and he died in two days after admission. So that you see that the fatality of rheumatism in early life is a very different thing from the fatality of rheumatism in adult life, and it is the children's hospitals which have demonstrated that fact.

In addition to this large mortality there is a great, one might say an enormous, amount of chronic heart disease caused by rheumatism in childhood; chronic heart disease which makes its appearance by-and-by for treatment in adult life, but the seed of which is sown in childhood. Many of the cases of rheumatism in childhood which survive have yet their hearts so severely crippled, that practically they have been deprived of many years of life. They never entirely recover, and are capable of but little exertion, and die long before their time. So that the cardiac part of rheumatism is really of immensely more importance than the arthritic part, and it is in the children's hospitals that this fact has been made out.

Now what are the cardiac affections of rheumatism? What is there about rheumatism which makes it so destructive, especially in early life? It has long been recognised that rheumatism has a pernicious influence upon the heart, and that endocarditis and pericarditis are not infrequent results. The endocarditis and pericarditis have been carefully studied. But one cannot help saying that this view that the cardiac affections of rheumatism simply concern the endocardium and the pericardium is far too superficial a view. It is *not* simply the serous membranes that suffer. What is probably of most importance is the condition of the cardiac muscle, and the cardiac muscle has been

far too much neglected. The physiologists tell us that it is the cardiac muscle itself which keeps the heart going; they find the prime cause of the action of the heart in the muscle of the heart itself. It is surely a curious thing that we have been so occupied with endocarditis and pericarditis, and have thought so little about the muscular wall. But while there are plenty of observations on pericarditis and endocarditis, the muscular substance is not quite so easily investigated, and therefore has not received the amount of attention it deserves. It has been recognised somewhat more of late that in the cases which prove fatal there is frequently a very considerable alteration in the structure of the muscular wall of the heart. My friend, Dr. Poynton, who has been examining these for some little time, and has made careful sections of the cardiac muscular wall by the most approved histological methods, some of which are quite recent, has shown that, in some cases at all events there is very decided evidence of the implication of the muscular structure of the heart, not simply as an extension from the endocardium or from the pericardium, as was supposed before, but that there is evidence of inflammatory change in foci throughout the entire thickness of the cardiac wall. He finds that there is hyperæmia, there are foci of small cells, there is some evidence of degeneration of the muscular fibre, which in parts loses its striation, and often shows fatty degeneration. If this is confirmed by other observers it will turn out to be the most important part of the action of rheumatism, for the heart is the vital organ; and if the cardiac muscle is attacked in this way by the rheumatic poison, it is a fact of the very first importance, and one of which the importance has not been sufficiently appreciated.

Now, is there any clinical evidence leading us to believe that such changes do occur in many cases? The evidence of which I have spoken already is post-mortem evidence. Is there any evidence in clinical investigation that the cardiac wall is affected in this way? Well, to that one must reply that there is such evidence, but it is not evidence which is obtained by means of the stethoscope. On the whole the stethoscope has been too dominant, and we have all relied too much upon mere auscultation, and far too little upon other means of investigation. It is exceedingly desirable that we should all acquire the habit of leaving the stethoscope to

the last, and of adopting that excellent rule that Sir George Humphry used to teach his students—"Eyes first, fingers next, *ears last*." There is no method of examination which I think, as a rule, is so imperfectly carried out as the percussion of the heart. I have had some opportunity, in my capacity as examiner, of testing the matter and ascertaining what are the ideas as to cardiac percussion of the men who come up for their final examination. One can only say that, as a rule, their ideas on the subject are exceedingly vague and unsatisfactory, and I think it only reflects the general vagueness and unsatisfactoriness of the usual practice of cardiac percussion. For I think you will have observed, as I have, that when the physician goes round the wards, and his house physician shows him a case of subacute rheumatism, with very little arthritis and very little pyrexia, the physician at once uses his stethoscope, and if there is no murmur he is satisfied. But, as a matter of fact, he has no right to be satisfied with that examination; every such heart (and, indeed, the heart of every patient) ought to be investigated first by inspection, by palpation, and by percussion before the stethoscope is employed. If that were always done views about certain conditions of the heart would be different from what they are in many quarters now.

What is the object of percussion of the heart? A student will reply "to find out the superficial cardiac dulness." "Well," you answer the student, "what is the value of the superficial cardiac dulness?" And one is obliged to say that it is of no use as far as the heart is concerned. It is of some value as far as the left lung is concerned, indicating whether it has shrunk or whether it is emphysematous; it tells you how much of the heart is uncovered by lung. But it is of little or no value as regards the heart. The prime object in percussing the heart ought to be to find out how big that heart is. And really when you come to try it for yourself, you will be surprised to find how comparatively easy it is.

I do not think it is yet appreciated by the great majority of the profession, that the normal cardiac dulness extends to the right of the right border of the sternum, in the fourth intercostal space, by about one finger-breadth; that the dulness of the right auricle can be detected always in the normal heart in the fourth intercostal space on the right

side, close to the sternum. Now, that is a point of the very greatest importance in diagnosis sometimes, because in conditions of disease which cause the right heart to be over distended, you will find a very considerable enlargement of the area of dulness to the right of the sternum in the fourth space. For instance, in cases of mitral stenosis, in cases of acute bronchitis, in cases of acute pneumonia, you may find that the dulness, instead of being one finger-breadth in the fourth right space, is two, or sometimes even three. And that is one of the best indications of the necessity for affording relief by the loss of blood, in one way or another.

Then again, the size of the left ventricle is a matter of very great importance, not only in rheumatism but in many other conditions. There is little difficulty in determining the size of a heart if you only observe certain conditions. One is that you do not put the slightest trust in pleximeters, but percuss simply with your own fingers. Another is that you use only light percussion. There seems to be a confused idea that because the dulness which we are trying to discover is what is called "deep" dulness, therefore you must percuss forcibly. The effect of that is simply to bring out the pulmonary resonance and to make the determination of the cardiac dulness much more difficult.

But if you think of it, the edge of the heart on both sides is tolerably thick, and the change of note, as you pass over it in light percussion, is generally quite well marked. So that it is possible, with very little difficulty usually, to say with considerable accuracy, though not with mathematical accuracy, where the right border of the heart is, and where the left border is. The point to keep in mind is that you must ascertain it along a transverse line in the fourth interspace on either side, below the nipple; also in the fifth, and sometimes in the sixth also, on the left side.

If then, starting from the limits thus discovered, the percussed finger be placed in a line sloping upwards and inwards with the percussed part (but no other part) firmly pressed on the chest wall it is usually quite easy to detect the rising edge of the left ventricle and of the right auricle.

But to return to the question of rheumatism, what I want to say is that by an examination of the heart conducted in this way in cases of rheumatism, even where there is very slight pyrexia,

and hardly any arthritis, you will almost invariably find a certain amount of dilatation of the heart. The heart is almost always bigger than it should be, and it is bigger especially in the left ventricle. At first the right side is usually unaltered, but the left ventricle is almost always dilated in an attack of rheumatism. Feebleness and diffusion of the cardiac impulse, and a shifting of it to the left, with shortening and feebleness of the first sound are further indications. This dilatation will often, in favourable cases, completely recover when the patient recovers from his illness. In less favourable cases the enlargement may remain permanently. It is an evidence of some toxic influence on the wall of the ventricle. Many years ago Dr. Gaskell published observations on the action on the heart of the frog, of lactic acid, and of sodium hydrate, using in each case a dilute solution of 1 in 10,000. He found that whereas the action of sodium hydrate was to cause contraction of the ventricle and of the arterioles, the action of lactic acid in the same degree of dilution was to cause dilatation of the ventricle and dilatation of the arterioles, so much so that finally the heart stopped in diastole. So that it is clearly possible for a toxic influence to dilate the heart, even without the production of actual myocarditis, and if myocarditis is present, as it certainly has been in some of the severe cases which have been examined post mortem, then one sees still more clearly why there is acute dilatation in rheumatism.

Now, passing from that, we come to the question of pericarditis. The rub in pericarditis is easily recognised, and it is of course a valuable sign. But here again one has to complain that the stethoscope has been too dominant. It is true that it is recognised that there is often an increase in the precordial dulness in pericarditis, but the explanation given of that dulness is unfortunately, in the main, erroneous. It is assumed—and there may be a certain amount of truth in the assumption at first—that in pericarditis a fluid effusion is poured out into the pericardium. It is further stated that this fluid increases until it distends the pericardial sac, so that then the patient's heart, unaltered in size though with inflamed surface, is in the midst of a large quantity of fluid effusion. In consequence of this there have been various interesting discussions, which you will find in writings on the subject, as to whether the apex

is raised or lowered, or remains in the same position. The curious point is that when you come to investigate the fact post mortem in many cases there is no fluid at all, and in others quite a small amount. I would not say that there may not have been a larger quantity of fluid at first, but if you refer to the analysis of the 150 cases of which I have spoken (which you will find at page 445 of the *Médecino-Chirurgical Society's 'Transactions'* for 1898) you will find that in three-fourths of these cases there was an adherent pericardium, that only one of the 150 cases is stated to have had six ounces of fluid in the pericardial cavity, and that in only twelve cases out of 150 was there more than two ounces of fluid in the pericardium. What is it, then, which causes this dulness? What causes this dulness is an enormous dilatation of the heart itself. Now, even in recent writings on the subject you will find it assumed, and even asserted, that the increase in precordial dulness in a case of rheumatic pericarditis is due to effusion into the pericardial cavity. But if you will watch cases which prove fatal you will find that in some of them there is practically no fluid at all. I am thinking now of a recent case which Dr. Poynton has published, under the care of Dr. Phillips (who kindly allowed him to use it). Dr. Poynton watched the enlargement of the heart from day to day by percussion, and found that the dulness finally extended even to the right nipple, and into the left axilla. He expected to find a certain amount of fluid in the pericardial cavity, and provided himself with some bacteriological apparatus to investigate it. When the post mortem was made there was practically no fluid at all, and the whole of this great extension of dulness was caused by a greatly dilated heart and a moderately thickened pericardium. There was a very little fluid in the meshes of the plastic exudation. Therefore in a case of pericarditis you may have the whole heart covered with plastic lymph, but with very little fluid. The outline of the precordial dulness, as described in the books, is taken mainly from certain injections of fluid into the pericardium in the cadaver, on the assumption that the dulness of pericarditis is evidence of fluid effusion. The outline of the pericardium thus distended in the cadaver was supposed to indicate what ought to be found in clinical observation of

pericarditis, and you will find drawings on this basis copied into the latest articles on the subject. As a matter of fact, that is entirely erroneous.

The pear-shaped outline sometimes described is not found in pericardial effusion. On the contrary, the outline of dulness in the left third and second spaces is extended to the left, as fluid collects around the great vessels, and the entire left border of the heart is also shifted to the left.

Many years ago Sir William Gull taught at Guy's Hospital that rheumatic pericarditis was apt to be associated with great dilatation of the heart, which was often mistaken for fluid effusion. I am sorry to say that that teaching does not appear to have made its way into the mind of the profession. I only discovered it myself in a footnote to a paper of Dr. Goodhart's written many years ago, dealing with the question of acute dilatation of the heart in scarlet fever. In that footnote Dr. Goodhart says that Sir William Gull had been in the habit of teaching at Guy's this particular fact. As far as I can make out, nobody has credited it from that time to this. And yet it is a fact that in rheumatic pericarditis there is a very large dilatation of the heart, and that that dilatation is responsible for the greater part, often for nearly the whole, of the increase in the precordial dulness. Of course in this I am not speaking of suppurative pericarditis at all, but of rheumatic pericarditis only.

With regard to endocarditis, it has been noticed in the examination of children affected with rheumatism, that it is the mitral valve which is almost always the first part attacked, also that the mitral valve is very apt to suffer in rheumatism. In the 150 fatal cases that valve was involved in all but one. Though of course it is not always affected, yet in a large proportion of the cases which recover it does suffer more or less beyond any question.

What are the clinical signs which show the first effects of endocarditis? That also has been worked out in children, for it is in children that you get the commencement of rheumatic endocarditis, and it comes out in this order:—the first thing in nearly all cases is a systolic apex murmur. The dilatation may be made out before that, but the first auscultatory sign is a systolic apex murmur. The next thing to be noticed is, in many cases, that there is an apparent reduplication of the

second sound at the apex, not at the base. The third is that the second element of that reduplication may become converted into a short blowing murmur. The fourth is that before long a short blowing presystolic murmur may be heard in nearly the same situation. I have not time now to go into the explanation of these signs, but that is the order in which the clinical evidences of endocarditis develop themselves in children.

The study of the disease in children has proved the frequency of these cardiac rheumatic changes, and has shown that they occur often with very little arthritis, or that arthritis may be absent altogether. Thus light has been thrown on the genesis and method of development of chronic heart disease as we find it in the adult. In the adult, as you study cases (for instance) of mitral stenosis, it is possible to enunciate a theory, which indeed I have heard enunciated by one of the most distinguished physicians now alive, that mitral stenosis is in many cases a congenital affection. The study of disease in children soon shows that that view cannot be correct. There is no mitral stenosis in infants. If it were congenital, of course the evidence would be seen in infancy. It is never congenital. But in the early years of life as you study the effects of rheumatism upon the heart you come upon this series of changes, which show a great deal of implication of the mitral valve, and one understands how in process of time—and you can watch the process more or less completely in different patients—the affection of the mitral flaps results in a contraction of fibrous tissue and in a slow narrowing of the orifice. And it must be pointed out that although a presystolic murmur is not uncommon in a child, it is not the typical presystolic murmur which you meet with in adolescents, it is not that rough churning murmur with which we are familiar, it is usually a short murmur and a blowing murmur, although of presystolic time. It is only as children grow up that this murmur becomes more and more vibratile, and finally develops the typical characters which we know in the murmur of mitral obstruction in people from twenty to thirty-five years of age.

Then there is another point of importance. The dilatation of the heart, to which I have drawn attention, is in many cases permanent, and is increased by later attacks of rheumatism, until in some cases the heart is enormously dilated. I

think no one who has not studied the disease in children has any idea of the enormous hearts which some of these rheumatic children have before they die, extending nearly to the right nipple, and three or four finger-breadths outside the left nipple to the middle of the left axilla. In cases not quite so bad as that the heart remains large, too large for the individual, and almost certainly with a systolic apex murmur. When such cases present themselves to us later in life we call them cases of mitral regurgitation or adherent pericardium, and we assign the mischief to the regurgitation at the mitral valve, or to the hampering effect of pericardial adhesion. Whatever truth there may be in both these explanations, and there is some truth in the first, and perhaps in the second when there are external adhesions also, it seems pretty evident that these are not the main factors.

It is probable that the main factor in such cases is a permanent dilatation, the result of successive acute dilatations in rheumatic attacks, until at last the heart is left much too large, possibly with an adherent pericardium, and certainly with a systolic apex murmur.

Thus in many ways the study of rheumatism in children's hospitals has thrown light upon the genesis of chronic heart disease as we meet with it in the adult, and it has also shown us that though the disease is not a fatal one in itself in adult life, it is one of the most serious diseases that affect the child. That is a fact which is certainly not sufficiently appreciated.

I have not time to describe to you now the other varieties of rheumatic manifestations in the child. I will remind you that there is pleurisy, there is pneumonia, there are the subcutaneous nodules which are so characteristic of rheumatism in the child, and of which you will find a full description and drawings in Dr. Cheadle's *Harveian Lectures*. There is the question of erythema and other skin lesions, and there is the very important question of the relationship with chorea. Another point is that concerning tonsillitis. All these are manifestations of rheumatism in childhood, most of which are much more rare, and one of which (the nodules) hardly ever occurs in adult life. Therefore the rheumatism of childhood is a disease differing in many respects from the rheumatism of adults. And it seems to me, as I said

at the commencement, that we began the study of rheumatism at the wrong end; instead of describing rheumatism as it occurs in the adult, and then adding certain particulars which we are pleased to call its peculiarities in childhood, as is done at present, we ought to describe the disease as it appears in the child, and then show how these peculiarities or features of the disease become modified, some being dropped, and others made more prominent with the approach of adult life. That point of view I think is the only one which will enable us to really understand what rheumatism is, for when we see the virulence of rheumatism in childhood, its frequency, and its variety, we shall see that no theory of perverted metabolism, no lactic acid theory, no uric acid theory, can possibly explain it. Nor can it be explained by any simply nervous theory. Its manifestations demand something much more definite, and the only theory which will at all explain it as it appears in childhood is a microbic theory. A microbic theory would explain its manifestations, and we may think of the analogy of such diseases as diphtheria, where the germ produces a toxin which has a selective action on certain parts of the body, with certain very definite results, such as occurs again in tetanus, the toxin having an enormously powerful action on certain parts of the nervous system. We see that the probability is that rheumatism is a toxæmia, the toxic agent of which is probably a poisonous substance produced by a microbe of some sort.

Again, when we think of the comparatively sudden onset of rheumatism, it is seen to be like that of an infection. When we think of the fact that it is accompanied by pyrexia, and in many cases by a characteristic eruption, that it is more virulent and more frequent in childhood than in adult life, we see in all these things analogies to the eruptive fevers. And, although no one microbe has yet been proved to be the cause of rheumatism, the same is true of such diseases as measles, whooping-cough, and scarlet fever, which, surely none of us can doubt, are genuine microbic diseases.

Thus the study of this disease in hospitals for children is contributing largely to a true understanding of the real meaning of rheumatism; and I think that the clinical features which I have endeavoured to point out will be explained some day by the discovery of a microbe which has the power

of producing these results. At all events, we see that it is impossible, in view of the variety of its manifestations in childhood, to rest contented with any such theories of its origin as satisfied us when we were dealing only with the comparatively monotonous course of the disease in adult life.

Remarks upon the Treatment of Whooping-cough.—Francis Huber ('Archives of Pediatrics,' June, 1900) finds that in some cases, when intubation is not practicable, one of the following methods gives good results:—Sending the children much in the open air or out upon the water; giving more food as soon as vomiting is over; pushing forward the jaw, as in anæsthesia, in many cases has relieved the paroxysm and prevented the vomiting. A third method is the use of codeine in small and repeated doses to the verge of slight narcosis, sufficient to overcome the spasm of the glottis. A few cases treated in this way have been carried safely over the danger point. As a rule it is not necessary to employ the remedy for more than a few days or a week, at the end of which period the severity of the attack is relieved, food is retained, and the usual routine treatment may be continued.—*Medical Record*, June 23rd.

Treatment of Nævus and similar Congenital Neoplasms.—Holländer finds that the best method of treating inoperable cases is by means of "air cauterisation," that is, the passage of a current of hot air over the nævus, and its application until incision proves the blood-spaces to be empty, and the part to be mummified. The advantages of this method over the old thermo-puncture, etc., are—that one application is usually sufficient; that the operation is bloodless, and even the blood in the tumour is preserved to the body; that the cicatrix is hardly noticeable in mild cases; and that it is applicable to all parts of the body, even to the cavities. Contra-indications are—an exclusively subcutaneous position of the nævus; total capillary substitution, extending to the mucosa; and cases where extirpation would give quicker results. Photographs of the severest case of angio-cavernoma treated by the author within the last ten years, showing the condition before and after operation, are given.—*American Journ. Obstet.*, June, 1900.

A THURSDAY CONSULTATION AT ST. BARTHOLOMEW'S HOSPITAL,

JUNE 28th, 1900.

MR. WALSHAM,—This, I venture to think, is a very interesting case. The patient, a woman of thirty-seven, is markedly neurotic, and you will see has a lump in the right breast. In addition to that there is an ulcer in the course of the lymphatics leading to the axilla. The history is that six weeks ago she first noticed a small lump in the right breast, which has steadily increased in size, and has always been more or less tender. About twenty years ago she had some ulceration of her face, having in many respects the characters of old tuberculous disease. She had also what I take to have been tuberculous dactylitis. Her fingers are shortened by destruction of bone, and there has been the same condition about the ulnar epiphysis, and in the lower humeral epiphysis. She has no evidence of syphilis—no keratitis, there is nothing in the throat or pharynx so far as we can make out. In the right breast, in the upper and outer part, is a diffused ill-defined mass, which is hard and tender, slightly adherent in places. Fluctuation is felt at one place from which pus was withdrawn by a needle yesterday; it was a mixture of curdy pus and blood. That has been sent up to the pathological laboratory, but we have not yet had the report. I suppose if the matter is injected into a guinea-pig it will be two or three weeks before we can decide whether it was tuberculous. The ulcer which I mentioned is movable, its edges are soft, pale pink, over-hanging, and fairly sharp cut, and it is of roundish shape. There is some fulness in the axilla, and several large soft semi-fluctuating glands can be felt. She says the ulcer came as a small lump, very similar to another lump which there is now on the lower part of the breast. The breast is not adherent to the deeper structures; it is movable, and is perceptibly larger than the opposite one. The patient is intensely neurotic, and when we punctured the part it took four dressers to manage her, as she made such a fuss. I had it in my mind whether she had artificially produced the

ulcer, but she could not have produced the destruction of her fingers or that about her right elbow. I think, looking at the characteristics of the scar, that she has had tuberculous disease, and I have also come to the conclusion that she now has tuberculous disease in her breast. Perhaps it has not all the characters which one is accustomed to see in tuberculous disease, but tubercle of the breast is not a common disease; I have only seen four or five cases of it at the outside. If the examination of the specimen of material drawn off should show distinctly that it is tuberculous disease of the breast I shall have no hesitation in recommending the removal of the whole breast with the axillary glands.

Mr. BUTLIN,—I am of opinion that it is one of the infective diseases, and of the two, syphilis and tubercle, I should certainly think it is likely to be tubercle. The ulcer, however, is not quite typical of tubercle. It reminds one in certain respects of a tertiary syphilitic ulcer. I should think the pathological department will settle that. With regard to tubercle of the breast, it is a rare disease, but I have had the opportunity of seeing five or six cases. In the first, there was an abscess in a tuberculous woman, and there was no difficulty in the diagnosis. In the second case there was no breaking down at all, and nothing to guide one in forming an opinion. I removed the breast and axillary glands some years ago by a considerable operation. After the operation I carefully examined it and it did not look like tubercle, and it was not until I had sections cut that it was discovered to be tuberculous disease. Of course removal was the best treatment for the patient. She made a good recovery and there has been no recurrence. But she has had tuberculous outbreaks in different parts of the body. I saw another patient, a young lady, who had ulceration. She was thought to have malignant disease. As I had had experience of one or two of these cases I came to the conclusion, from the appearance and the family history, that it was a case of tubercle and I cut it out, as I thought freely. But a fortnight later I was sent for again, and I had to do a second operation, and since that time she has been perfectly well. I remember looking at Bryant's book on Tumours of the Breast and finding that he took a somewhat gloomy view of tuberculous disease of the

breast. My experience has been that if you cut the disease out freely—not so freely as cancer, but very freely, and take away the glands, you will get union by first intention and the prognosis is very good. I should not wait very long before doing the operation. It is a very extensive disease. I should take out the glands at the same time.

Mr. WALSHAM,—On the question of recurrence, you will remember that Sir Thomas Smith had a case here which he showed in consultation more than once. As far as I know there has been no recurrence since the breast was removed.*

Mr. D'ARCY POWER,—The possibility of it being syphilis passed through my mind, but looking at the scars which she has, I think the past disease was almost certainly tuberculous. I should do as Mr. Butlin suggests, remove the breast, and not wait a long time before doing so.

Mr. WALSHAM,—It will be interesting to examine a piece of that ulcer before operating, and it shall be done. No harm will occur from waiting a week or two.

Mr. BUTLIN,—I have a case of actinomycosis. My colleagues have not seen the patient since she was here two and a half years ago. I show you a drawing of the case at that time. There was induration extending over the lower part of the abdomen from the right to the left side almost as far as the iliac bone. There were a number of openings through which a quantity of pus exuded. When I showed her in February, 1899, my colleagues agreed that no operation should be practised, and thought I had better continue using iodide of potassium, which she had been taking in large quantities for some time previously. I continued it, but feeling afraid that an opening would form in the intestine or bladder I increased the dose until I was giving her more iodide of potassium than I have ever given to a patient. She took grs. 180 of iodide of potassium a day,

* The breast with the enlarged axillary glands has been since removed by Mr. Walsham, and the diagnosis of tuberculous disease confirmed. The axillary glands presented the typical appearances of caseating tuberculous glands. The breast was infiltrated with tubercle, which at one or two spots had caseated and broken down into an abscess. The specimen is being hardened in formalin, and will be placed in the Hospital museum. This will make the second specimen of tubercle of the breast contained in that collection. There is no example of it in the Hunterian Museum.

that is to say grs. 60 three times a day. The result has been marvellously good; she came back with her induration almost gone. The whole of the abdomen at one time was practically fixed. She came the other day with two tiny abscesses. They were opened yesterday morning, and some discharge was sent to the pathological department, and Dr. Andrewes' report is that the pus shows no signs of the characteristic actinomycoses. The patient has been under my care on and off for two years. The condition began apparently with appendicitis on the right side. Once or twice she had some salivation with the iodide of potassium.

Mr. WALSHAM,—I think, as the patient is not cured, Mr. Butlin might be asked to show her again. I had a case of the kind which was left to me by Sir Thomas Smith. We thought the patient was going to get well; he went home but died there. The question is whether the treatment actually cures the patient.

Mr. HARRISON CRIPPS,—I have a recollection of this case, and it is altogether in a different state now to what it was before. Presumably the difference is due to the iodide of potassium, though possibly to the lapse of time.

I have been asked to show a case for Mr. Willett. It is that of a patient sixty years of age, who has a tumour in her right breast which she has noticed for about four months. I am told that there is a large lump in the breast, the glands in the axilla are enlarged, and the supra-clavicular glands are also implicated. Her general health is not particularly good; there is a certain amount of albumin in the urine, and she complains of a good deal of intermittent pain in the breast itself. The question raised here is not so much as to the nature of the disease, which is certain to be malignant, but whether it is a case which ought to be operated upon. It is not a particularly favourable case for operation, because the breast is extensively involved, and the glands above the clavicle are thought by Mr. Willett to be implicated, though I cannot satisfy myself from my own observation that they are. Moreover, the albumin in the urine is against operation. Still I advise that the breast be removed, and any obvious glands in the axilla taken away at the same time. Even on the ground of relieving pain it is worth the

patient's while to have operation done. Of course, before operating, I should satisfy myself that there is no extensive disease of the kidney.

Mr. WILLETT (who entered at this point),—There is one gland which I can move under my finger above the clavicle. I suppose there is no doubt about the diagnosis, and that the disease is one for removal by operation, but there are unfavourable features about the patient herself. She is sixty years of age, has a very broad arcus senilis, her arteries are rigid, and she has a small amount of albuminuria. She seems not likely to live very long. The operation is one which, if it is to be a thorough and complete one, may well tax the endurance or vital powers of the patient. I should be disposed to operate, but it is because I felt that the risk was greater than usual that I thought a consultation should be had on the case. The growth is not increasing very rapidly. She has known of it for more than a year, and it is only just on the point of commencing to ulcerate. I think her outlook would be very miserable without operation.

Mr. BUTLIN,—If she were a young woman and in good health I should look upon the case as a very bad one. There are several bad features about it. There is a deep depression close to the sternum, and I think before long that will be followed by involvement of the periosteum of the sternum itself. Therefore I think operation would not save her from recurrence, and it would accordingly become a question of palliative treatment. I think it would be well to get rid of the breast. I should not do a big operation; at all events at one time. I should remove the breast by one operation, and leave the glands until another time.

Mr. WALSHAM,—I regard it as an unfavourable case, but looked at all round I should recommend removal of the breast with the glands from the axilla if I did anything. The question turns, in great part, upon the amount of albumin. She has not been in the hospital long. The albumin sometimes disappears from patients who are kept in the hospital a week or two. If the albumin cleared up I should be inclined to do an extensive operation so as to get rid of the disease locally.

Mr. D'ARCY POWER,—I think we should be justified in removing the breast and taking out the axillary glands at the same time.

Mr. BUTLIN,—Where you have a bad subject and you do not think the operation is going to cure, it is only a palliative measure. It is possible that removal of the glands might be a success for some time, but one would not do one of these enormous operations, because she might die of it. I think it would be safer, and give her quite as reasonable a chance, to divide the operation into two stages; she would bear it better than a prolonged and extensive operation at a single sitting.

Mr. WILLETT,—There is a doubt whether that which we feel close to the margin of the breast is an axillary gland or thickened and diseased lymphatic vessels.

Mr. BUTLIN,—It is in the axillary spur of the breast and would come away with the tumour.

The Medical Treatment of Gall-stones.—

Stevens ('Cincinnati Lancet Clinic') holds that it is frequently difficult and often impossible to make a positive diagnosis of gall-stones, and that there is no known medicine by which their solution can be effected. Introducing a cannula into the gall-bladder for the purpose of sounding for stones, introducing solvent remedies or withdrawing fluids, is dangerous as compared with the amount of good likely to be obtained, and should not be practised. The same is true of massage in emptying the gall-bladder; and he says that there is only one safe and radical cure, that is surgical operation. There is, nevertheless, what may be called a medical treatment of gall-stones that is often efficient in the relief of pain and other symptoms, and satisfies the patient that something is being done. Hot water applications may do some good, but the use of opiates at the beginning of the attack is to be avoided. Constipation and intestinal sepsis should be treated. The administration of laxative medicines is often followed by so remarkably favourable results that the patients sometimes believe they have been cured, and the physician is also deceived. Salines and oils tend to lessen the congestion, and cause an expulsion of septic products, and their use is, therefore, legitimate.

Journ. Amer. Med. Assoc., June 23rd.

THE RESPIRATORY AFTER-EFFECTS FOLLOWING THE INHALATION OF ETHER.

A discussion at a Meeting of the Society of
Anæsthetists.

The President, Dr. J. FREDK. W. SILK, in the
Chair.

(Continued from p. 240.)

Schema of Discussion on "The After-effects of Ether Inhalation upon the Respira- tory System."

It was suggested by the Council that the following were the chief points which it was desirable to discuss; but it was not, of course, expected, nor was it advisable that each speaker should refer to every point.

I. Frequency with which such after-effects are met with:

- (a) In the Larynx and Bronchi.
- (b) In the Parenchyma of the Lungs and in the Air-cells.
- (c) In the Pleura.

II. Limit of time during which there is a liability to the occurrence of such complications.

III. Precise nature of such complications, *e. g.* Bronchitis, Broncho-pneumonia, Pneumonia, Pleurisy, etc.

- (a) Details of the several complications, *e. g.* Age, Previous History (especially former respiratory troubles), Onset, Course, Termination (necropsies).

It is most important to cite cases, with particulars as to temperature, clinical signs, etc.

IV. Actual time of occurrence of the several types.

V. Contributory causes:

- (a) Pre-existing disease or auto-infection from disease of Jaws, Mouth, Tongue, Throat, Pharynx, and Abdomen.
- (b) Amount of Anæsthetic used, and the method of its administration. Possibility of infection from inhalers.

- (c) Nature and duration of operation.
 1. Extensive Breast Operations.
 2. Operations involving considerable Hæmorrhage.
 3. Operations on the Mouth and Upper Air-Passages, with aspiration of blood, mucus, or organisms.
 4. Operations in the deep cervical region leading to trophic changes.
 5. Exposure during operation.
- (d) Exposure after operation.
- (e) Influence of time of year, surrounding temperature, etc.

VI. Prophylaxis.

The SECRETARY read the following note from Mr. Carter Braine:—"With regard to the frequency of after-effects upon the respiratory system after the administration of ether, I personally am of opinion that they are rarely met with provided the ether has not been given in an unsuitable case, or the administration of ether persisted in when there are obvious signs present that it should be discontinued, and chloroform substituted. At Charing Cross Hospital we have recently commenced keeping an anæsthetic register, and pending this discussion I have carefully gone through the ether administrations, 900 in number, and find under "after effects" no mention of any case of bronchitis or pneumonia. I have also looked over my own case-books and find that in 3,480 cases there is only one case of pneumonia recorded. Adding the two amounts together the total is 4,380 ether administrations with only one case of pneumonia recorded. These are cases in which ether was given throughout the entire operation. The ages of the patients have been from seven months to eighty-six years. I have my doubts whether this one case of pneumonia was due entirely to the ether. M—, æt. 16. Operation, tonsils and adenoids. Position, head extended over the end of table. The hæmorrhage was very free when the tonsils were excised, entirely filling up the back of the pharynx and covering the palate to the level of the incisor teeth; fearing blood might enter the trachea I turned him on his right side, so that the adenoids could be removed in that position; this turning from the dorsal to the lateral position was not done smartly enough.

I am of opinion that blood did enter the larynx after all the endeavours to prevent it. Patient was then placed in bed on his right side, breathing was of a very moist character and the countenance bluish; he developed broncho-pneumonia from which he eventually recovered. Patient was not under ether more than two minutes, so that I cannot think the broncho-pneumonia was entirely due to the ether. I discovered afterwards that the windows and door of the room were opened after the operation to get rid of the smell of ether, although it was winter time. This exposure may have had some slight influence, but the surgeon and myself are of opinion that the pneumonia was most probably due to blood having entered the bronchus.

"I have witnessed a post-mortem on a case of death from ether administration due to œdema of the glottis, in a patient the subject of lymphadenoma of the neck and pharynx; the ether was given by a house physician, and the case was one in which ether was absolutely contra-indicated. The patient died of asphyxia before any operation was commenced, and tracheotomy, which alone could have saved the patient, was not attempted. I secured the specimen of the larynx and exhibited it in its recent state before this Society; the œdema of the glottis was well marked.

"It has always been a puzzle to me why patients lying on the operating table in a draughty theatre with the chest or abdomen exposed for two to three hours or even longer do not get bronchitis or pneumonia; they are placed on wet towels, and solutions are occasionally poured over the part under operation. It is true these solutions and cloths are warm at first, but towards the end of the operation the patient is lying upon cold cloths from the buttocks to the neck, and the whole of the back is quite chilly to the touch. It would really seem as if there was something protective in being under an anæsthetic in such surroundings. Operations are becoming more and more prolonged, so that it will soon become imperative for hospitals, in order to be considered up to date, to possess a hot water operating table to lessen the effects of exposure in prolonged and desperate operations. Patients after operations also require protection from exposure, more especially in private practice. The friends are

almost certain to throw up windows and open the door, with the object of getting rid of the smell of the anæsthetic, directly you have left the house; and, unless cautioned, are liable to expose the patient to a thorough draught and lowering of temperature of the room; in such circumstances any respiratory after-effects are more likely to be due to the after exposure than directly to the influence of the anæsthetic. If ether be given haphazard to every case that presents itself for anæsthesia, then such after-effects as bronchitis and pneumonia will be occasionally met with, but by a judicious selection of our cases, and more especially by discontinuing ether and substituting chloroform directly we find, through the presence of cyanosis, free secretion of mucus, and moist bubbling breathing, that we have been deceived in our opinion of the case being suitable for ether, then I consider such after-effects as those under discussion will rarely, if ever, occur."

The SECRETARY then read the following letter from Dr. Pounds:—"My experience is that ether is by far the best and safest anæsthetic for serious and prolonged operation work, but that it is occasionally followed (one per cent. or less) by lung trouble. This lung trouble (broncho-pneumonia) is due to a purely mechanical cause in the first instance, *i. e.* the physiological *hypersecretion* of mucus plugs up the smaller bronchi, giving rise to secondary lobar pneumonia.

"Belladonna or atropia given *early* in these cases, as suggested by Ringer, is clearly one remedy, and since adopting this method of treatment I have had no serious trouble.

"The giving of ether by the Junker inhaler, as modified by my friend Dr. Copestake, of Derby, does certainly seem to give rise to a diminution in the secretion of mucus."

Mr. CORNER said that at Dr. Tyrrell's request, the other day, he rapidly looked through notes of cases in which respiratory complications had ensued after the administration of ether, and was able to present three cases for the consideration of the Society.

W. K—, male, æt. 28, and a member of the Cape Mounted Police, was admitted to St. Thomas's Hospital with extensive varicose veins and phimosis. He was of splendid physique and showed no signs of disease except the above

anywhere. He admitted to somewhat free indulgence in alcohol. On April 3rd, 1899, the veins of the right leg were excised. Ether was given and the patient was a long while going under. He was under the anæsthetic for about an hour and a quarter. It was very difficult to keep him "under" and a large quantity of ether was used; it was difficult to say, positively, the exact amount. On the night of the first day after operation his temperature rose to 100° and he complained of a cough. On the night of the second day the temperature reached 102·8°. The temperatures attained during the succeeding four days were 102·2°, 101·8°, 102°, and 103·2° respectively. On the next day, *i. e.* the sixth since the temperature rose, an apparent crisis occurred, the temperature dropping from 103·2° to 99° in twelve hours. From the seventh day onward the temperature was normal. During the period of pyrexia the patient was obviously very ill and brought up quantities of muco-purulent sputum, occasionally streaked with blood which was more bright than rusty. His legs healed perfectly and without any beads of pus. His chest was examined carefully and presented no more physical signs than those of bronchitis. The sputum was on two occasions examined for tubercle bacilli, which were not found. The patient left the hospital after being in twenty-one days. On May 18th he was readmitted to have the other leg operated on. It was now found that there was considerable loss of power and wasting of the muscles of the thumb and hypothenar eminence of the left hand. This loss of power came on after the first operation and during the patient's more severe illness was unnoticed. It was probably an example of post-anæsthetic paralysis. On May 28th the veins of the left leg were excised in the ordinary way, but from the previous experience chloroform was administered instead of ether. Again it was noticed that the patient required large quantities in order to keep him "under." After operation the patient was perfectly comfortable and had no respiratory trouble. The highest temperature recorded was 98·8°. In the first operation the apparatus was sterilised and perfectly clean, and, so far as is possible, influenza may be excluded.

J. K—, female, æt. 51, was admitted to St. Thomas's Hospital, on June 11th, 1899, suffering

from sub-acute intestinal obstruction. She was a well nourished woman giving a past history of occasional attacks of bronchitis. On the day of admission she was operated on and colotomy was performed. Ether was administered in a newly-cleaned and sterilised inhaler. Patient took the anæsthetic well, but secreted mucus freely. Immediately after operation her temperature rose from 97° to 101° and she complained of much cough. On the second day the physical signs of bronchitis were present and much muco-purulent sputum was brought up. The temperature reached its highest point, 102° , on the third day after operation and from thence slowly declined, reaching normal on the eighth day after operation. The bronchitis also declined after the third day, but was very acute up to that date. The condition of the wound in no way accounted for the temperature and influenza seemed unlikely. On September 7th the patient was readmitted, and on the 13th was taken to the theatre and a lateral anastomosis done. This operation took over twice as long as the former, chloroform was the anæsthetic used and the patient was perfectly well after operation. The temperature rose to 100° immediately after and was never above 99° later. On September 22nd the artificial anus was closed. Chloroform was administered and the patient had no respiratory symptoms, but the temperature rose from suppuration.

D. N—, male, æt. 62, police pensioner, was admitted for inoperable carcinoma of the rectum. He gave a past history of gout and rheumatism. Colotomy was performed under ether. Bronchitis developed within twenty-four to thirty-six hours of the operation, but was not severe. The pyrexia lasted seven days, the maxima ranging between 101.4° and 102.2° and was markedly irregular in type. So far as possible all extraneous sources like exposure, infection, etc., could be excluded. The physical signs of bronchitis alone were found.

In the first two cases the ether was not administered by any of the staff anæsthetists. The first presented the greatest difficulty in rendering the patient even tolerably quiet, and in the second the patient was soaked with ether.

With regard to the special schema devised by the Council the following points are illustrated by the first two cases:

In both cases the bronchi alone were

affected, and in both cases the patient was very ill.

2. In the woman the bronchitis began almost immediately after the operation; in the man within forty-eight hours.

3. The ages, etc., are given above. In the male there was only a previous history of robust and perfect health; in the woman there was history of frequent attacks of bronchitis.

4. Nil.

5. (c) In the male both operations were of one and a quarter hours' duration; in the female only about forty minutes. There was no exposure either before, during, or after the operation. The time of the year was April to August.

(d) Both cases were the first operations of the days, and sterilised apparatus (Clover's) was used.

Another point which was not mentioned in the scheme but which he would like to introduce was regarding the temperature. He would pass round a temperature chart because it introduced a difficulty, that of excluding influenza, and showed it very well. The upper case was that of a man aged twenty-four, who had a double radical cure for inguinal hernia. He had never had bronchitis, but had had influenza characterised by severe headache, severe pain in the back, limbs, eyes, etc. There was no suppuration, yet his temperature rose, within twenty-four hours of operation, to 102° , and continued high for a week. In the second case the chart is that of the man, whose case was read to you, who had varicose veins, in which the temperature began to rise on the second day after operation, and reached the normal on the ninth day, giving six or seven days of pyrexia. But the point which made those cases interesting was the fact that two had ether first and then had chloroform under similar conditions. Both of them were very ill after the ether from bronchitis, which started almost immediately after the administration of the anæsthetic, whilst after the chloroform they were perfectly well. One had chloroform once for the same time as that taken for the first operation (ether), and the other had it twice for far longer operations, but in neither patient was there the least untoward result.

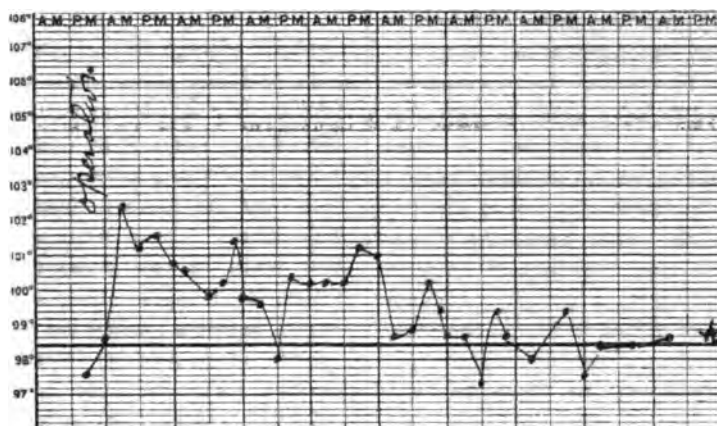
In conclusion, Mr. Corner said, so far as he could find out bronchitis and broncho-pneumonia possibly had occurred in the hospital practice. No

cases of pneumonia, laryngitis, pleurisy, empyema, etc., could be found. The bronchitis cases occurred when every precaution for sterilising the inhaler and avoiding undue exposure of the patient was observed. And, as they occurred after ether and not chloroform inhalation in the same cases, the presumption was that the ether

first operation was out of the hospital for about a month, during which he was not regaining good health, because he was addicted to intemperance. The other patient was out of the hospital about two months with inoperable carcinoma.

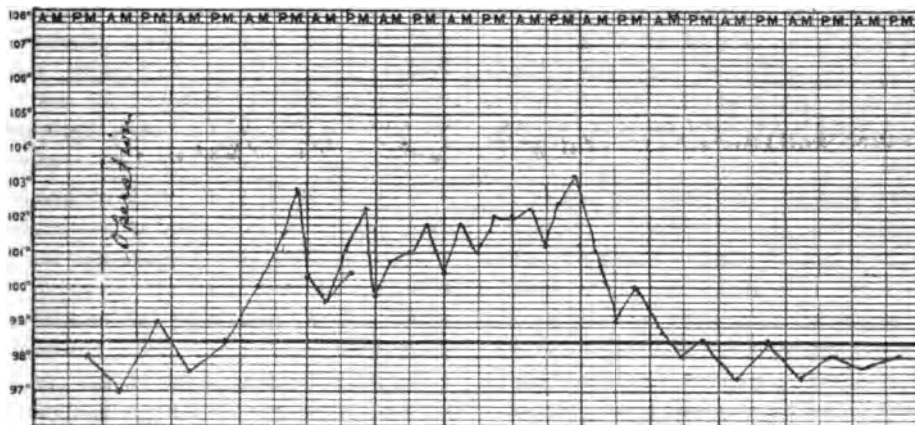
Dr. DUDLEY BUXTON asked at what period of the year the operations were done.

Days 1 2 3 4 5 6 7 8 9



I. M—, ♂, æt. 24. Operation: radical cure for double inguinal hernia. No bronchitis or signs in lungs. Influenza characterised by headache, pains in back, limbs, eyes, etc. No suppuration. Anæsthetic: ether.

Days 1 2 3 4 5 6 7 8 9 10 11 12



W. K—, ♂, æt. 28. Operation for varicose veins. Anæsthetic: ether. Bronchitis. No suppuration.

was responsible. Ether bronchitis had been noticed to be more frequent after unskilled than skilled administration of the anæsthetic.

Dr. DUDLEY BUXTON asked Mr. Corner whether the patients, between the first and the second operation, were lying in the hospital.

Mr. CORNER replied that the man after the

Mr. CORNER replied that the man was first operated upon in April, and the second time in May. The woman was first operated upon in June and later in August or at the beginning of September.

(To be continued.)

The Significance of Tubercle Bacilli and other Bacteria in the Sputum.—In the presence of symptoms and signs of disease of the lungs the discovery of tubercle bacilli in the sputum may be regarded as indicative of the existence of pulmonary tuberculosis. Failure to find the bacilli, even on repeated examination, cannot, however, be accepted as evidence against pulmonary tuberculosis. The bacilli can appear in the sputum only if breaking down of tubercles has taken place and communication exists between the ulcerated tissues and the air-passages. They are thus absent, as a rule, in cases of miliary tuberculosis; and, sometimes, in cases of early or even advanced tuberculosis careful and oft-repeated examination may fail to disclose their presence. On the other hand, the persistent expectoration of large numbers of bacteria is indicative of the presence of a cavity, although sometimes the number varies widely accordingly as the cavity communicates with the air-passages or not. In an address on the diagnostic and therapeutic significance of tubercle bacilli and other bacteria in the sputum, Brieger ('Berliner klinische Wochenschrift,' No. 13, 1900) points out that a factor of great importance in cases of pulmonary tuberculosis is the matter of mixed infection. There is reason to believe that to this is due principally the violent febrile movement so often observed in cases of advanced tuberculosis. Pure, uncomplicated local tuberculosis of the lungs is, it is thought, almost wholly unattended with fever, and the sputum is, besides, rather scanty.

The micro-organisms principally responsible for secondary infection are streptococci, staphylococci, and pneumococci, singly or in association. The hectic fever, with chills and night sweats, is believed to be due principally to streptococci, while to the staphylococci are attributed the suppurating and softening processes in tuberculous lungs. Influenza bacilli also are not rarely found in the sputum from tuberculous patients, and they are highly capable of aggravating the tuberculous process. The *Bacillus pyocyaneus* and the *Micrococcus tetragenus* are occasionally found in sputum from tuberculous patients. From the evidence, the rôle of mixed infection would seem to be the conversion of pulmonary tuberculosis into pulmonary consumption.

In the prophylaxis of mixed infection in cases of pulmonary tuberculosis especial attention should be given to the care of the mouth. Inhalations of aromatic oils, especially peppermint oil, probably exert a useful therapeutic influence in this direction. Fresh air rich in ozone and hydrotherapeutic measures may also be productive of good by neutralising the evil effects of mixed infection.—*Medical Record*, July 14th, 1900.

CHRONIC enlargement of lymphatic glands in the neck probably gives more trouble both to the medical man and to his patients than many other affections. There is no doubt that much of this is due to treating these so-called "glands in the neck" by the method loosely termed "painting with iodine." In a large majority of cases the final result of this orthodox and popular procedure is disappointment, and many cases drift on in a hopeless manner till the condition reaches a point where surgical interference becomes imperative. The cause of much of this vexation and trouble is not that iodine is useless to effect the object aimed at, but the drug is so prescribed that the diseased tissues do not receive the medicament. That this is the case can be shown, and can be demonstrated to be the fact, by applying the iodine prepared in such a vehicle that its absorption shall be rendered possible and probable. The medium required is found in the substance known as vasogen, and this useful solvent can be employed for the purpose of securing the absorption of such therapeutic agents as mercury, guaiacol, creasote, iodoform, etc. The reason of the great success which attends the exhibition of drugs applied in this neutral solvent vasogen is that this preparation renders drugs incorporated with it soluble in water, or emulsifiable with it, explaining the undoubtedly rapid absorption that occurs. From actual experiment and close clinical observation, the conclusion has been reached that in vasogen medical men have a most suitable and excellent agent to hand that is deserving of the widest recognition. Mr. E. J. Reid, of 11, Dunedin House, E.C., writes that he is prepared to send samples of the following drugs compounded with liquid vasogen:—Ichthyol, iodoform, iodine, guaiacol, beta-naphthol, creasote, camphor-chloroform, menthol, sulphur, and tar.

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*Specially reported for the Clinical Journal. Revised by the Author.

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A CLINICAL LECTURE

ON

COMPRESSION OF THE BRAIN AND MIDDLE MENINGEAL HÆMORRHAGE.

Delivered at Westminster Hospital,

By CHARLES STONHAM, F.R.C.S.,

Surgeon to the Hospital.

GENTLEMEN,--Compression of the brain may arise from a variety of causes, and the causes can be fairly conveniently grouped according to the rapidity and depth of the symptoms which they produce. The symptoms of compression may come on in a tumultuous fashion, that is to say, immediately after the injury, and may gradually progress until the patient dies. The causes which produce compression of this severity are usually a depressed bone or a large hæmorrhage, either from laceration of the brain, or from apoplexy. Of course the hæmorrhages from apoplexy practically never come under the surgeon's notice; whether they ever will do so is another matter. The symptoms of compression may come on more gradually, either being preceded by signs of concussion, or coming on after a distinct interval of time. Gradual compression of this nature is dependent in the great bulk of instances upon middle meningeal hæmorrhage. The reason why the compression symptoms should come on some time after the actual damage, when, of course, the artery must be torn, we shall see later. In the group of gradual onset come inflammatory effusion and suppuration, either within the brain, or suppuration outside the dura, or between the dura and pia arachnoid.

The least rapid form of compression of all is dependent on the growth of some tumour, such as a syphilitic gumma, or a mass of tubercle, or a definite new growth.

Before I go on to the symptoms of compression it will be as well to give you some account of middle meningeal hæmorrhage. Middle meningeal hæmorrhage nearly always takes place from the

anterior branch of that artery. Sometimes it occurs from a posterior branch, but that is a comparatively rare occurrence. The hæmorrhage varies in amount, the usual average being perhaps two or three ounces, but sometimes it is considerably more. The mechanism of the matter is, that any blow upon the head of sufficient severity tends to separate the dura mater from the bone. In the mere process of separation the middle meningeal artery may be torn across, but in the great bulk of instances the artery is torn across by a fissured fracture which crosses its groove. Sometimes the fracture only implicates the inner table, and sometimes there is no fracture at all; and it is in these cases that a mere separation of the dura mater tears the artery from its groove. Now if an artery were torn in such a manner, you would expect that the hæmorrhage would be immediately profuse, and that the patient would exhibit symptoms, at all events within a few minutes. The reason that he does not do so is that the blood has to come out under tension, and the contents of the skull have first of all to be compressed; this means, of course, that the bleeding must take place more or less gradually.

Another thing is that in the great bulk of instances the patient is concussed at the time of the injury. Cerebral concussion always implies great cardiac prostration, so that the propulsive powers of the circulation are at a very low ebb. The consequence is that there is practically a condition of temporary arrest of hæmorrhage, and it is when the patient begins to pull round from his concussion, as the heart is whipped up, as it were, in his recovery, that the blood begins to pump rapidly out of the vessel. The clot in middle meningeal hæmorrhage is very granular, and is shaped as much like a penny bun as anything else. It is far more adherent to the dura mater than it is to the bone. In the great bulk of instances the artery is carried away with the dura; so that in trephining, when you have taken out a piece of bone you have the blood-clot immediately underneath, and next would be the dura mater with the artery. The consequence is that an attempt to secure that artery is very likely to be futile. There are a small minority of cases where the artery is lying in a deep groove in the bone, or perhaps in a canal in the bone. In such cases it will not be carried away with the dura mater, and such cases are most

hopeful for treatment, because you have the artery right under you when trephining, and you can plug that canal; or if it be in a groove, you can pick the artery up and tie it. But this is not the rule.

The signs of compression may come on immediately after the injury, or they may be postponed, and it is particularly in middle meningeal hæmorrhage that they are postponed, perhaps for some hours, usually not more than one or two hours. You must remember that the patient may first of all show signs either of concussion of the brain, or he may show signs of laceration of the brain, and these gradually merge into the signs of compression. I have had one or two cases here within the last year where the patient was at first evidently merely concussed, and within three or four hours, when he came round from his concussion, his heart beat more forcibly, and he developed signs of compression from hæmorrhage from the surface of his brain, or from his middle meningeal artery. The case I referred to last month turned out to be one such as I had diagnosed—fracture of the base of the skull, with hæmorrhage outside the pia arachnoid and underneath the dura mater, the hæmorrhage being apparently due to rupture of a petrosal sinus. In that case before admission, the man never showed definite symptoms of compression at all, but had evident symptoms of contusion of the brain, and also symptoms of concussion when he was first picked up; then his symptoms gradually began to deepen, and he eventually died, practically from pressure. You will remember we did not have him here till quite late in the history, and he then developed definite signs of compression. If the concussion merges into compression, it may be difficult to differentiate the symptoms of the two conditions. But in the case of middle meningeal hæmorrhage, when the patient has a definite interval of consciousness after the accident, there may be no doubt about it; and it is exactly that interval of consciousness which is so important in the diagnosis. Concussion is signalled by the fact that it comes on immediately after the injury, and the patient tends to get better. It may be that he will not get better for some days, but still he tends to improve. In the case of compression there may be, or there may not be, an interval of consciousness. Then the symptoms of compression come on, and the patient tends to get much worse, and, unless surgery helps him, he will

inevitably die. The time at which he may die after the receipt of the injury must depend on circumstances. It depends first on the amount of the compressing force, but chiefly upon its situation. For example, in a case of apoplexy, if it takes place into his basal ganglia (the optic thalamus and corpus striatum), it is nothing like such a serious immediate business for him as it is if he gets an apoplexy in the region of his fourth ventricle or medulla. Even a small apoplexy into the fourth ventricle will probably kill a man in a few hours.

With the onset of symptoms of compression, assuming the patient to have had an interval of consciousness, he gradually begins to feel more or less lethargic. Probably he finds his head aches, he may have a feeling of dizziness, and he finds that his muscles decline to contract with any great vigour; in fact, he is gradually falling into a condition of paresis—mental and physical. His drowsiness and languor gradually deepen into a semi-conscious condition from which it is possible, at any rate in the early stages, to rouse him, but from which you cannot rouse him for any length of time. You may possibly get him to answer a question, but he does so perhaps after thinking for a minute or two, and very likely answers wide of the point. Even if he answers you in a manner which shows he quite understands the question, you will never get him to keep up a sustained conversation. He relapses pretty quickly into a condition of extreme indifference, and later on you cannot rouse him at all. His semi-conscious condition becomes one of pronounced unconsciousness, and that gradually deepens to coma, which terminates in death, unless the patient be relieved.

As a rule, in quite the early stages of compression, the pupils are equal. Very soon afterwards the pupil on the compressed side dilates, while on the opposite side it remains about normal. The dilatation of the pupil is due to compression of the third nerve. In cerebral compression from middle meningeal hæmorrhage, the dilatation of the pupil is on the side struck. For example, if a man has a blow on the right side of his head, and his middle meningeal artery is injured, it will be the right middle meningeal; that is to say, he gets a fracture at the point struck, crossing the middle meningeal groove, and tearing the artery on the same side; therefore it will be the pupil on that side which will be dilated. If his pupils are unequal, and

the inequality is shown by dilatation of the pupil on the side opposite to that struck, it shows the compressing agent is opposite the point of impact, and that nearly always indicates a cerebral laceration. It is to be remembered that this is a very important point as regards the surgical treatment of these conditions. The pupil at first may react to light, that is to say, in the stage of semi-unconsciousness, when you may be able to rouse the patient and when the pupil is only beginning to dilate; but later on, when dilatation is fully marked and semi-unconsciousness has passed into complete unconsciousness, you will get no reaction to light whatever. The conjunctival reflex also disappears, and there is then a certain degree of genuine paralysis. The condition of muscular languor that ushers in compression is very soon replaced by paralysis of definite groups of muscles, corresponding to that portion of the brain which may be compressed. In the case of middle meningeal hæmorrhage the paralysis takes the form of hemiplegia, the hemiplegia being on the opposite side to the dilated pupil, and therefore on the opposite side to the point struck.

In cases of laceration there may be a similar condition of hemiplegia, assuming there is a large amount of hæmorrhage with the compression; but the hemiplegia will be on the opposite side to the side on which the pupil is dilated, and on the same side as that struck.

The temperature in compression is usually raised; it begins to rise a few hours after the compression makes itself manifest, and may mount as high as 102° , 103° , or even 104° . As a rule, the higher it goes the more serious is the prognosis in the case; the surface temperature is also altered. On the paralysed side the temperature is raised, perhaps half a degree, perhaps a degree; this is due to the fact that not only is there muscular, but there is also vaso-motor paralysis. Vaso-motor paralysis, of course, gives rise to increased surface temperature. The skin is generally flushed and suffused, especially on the paralysed side; it is hot, and it very likely sweats. You see all these facts are in marked contrast to what happens in concussion. The sphincters are relaxed, and the patient very likely has retention of urine from paralysis of the bladder, and he passes his fæces under him. I need hardly point out that if the tension be not relieved by the catheter, the patient

very soon gets so-called incontinence of urine, that is to say, the overfull bladder tends to relieve itself by a perpetual dribbling of urine.

The breathing is irregular; it is also noisy and stertorous, and towards the end becomes much slowed, and stops before the heart stops beating. The reason of that we shall see presently.

The condition of the pulse is slow and somewhat full, fairly easily compressible, and towards the end it becomes irregular and extremely rapid from exhaustion of the centre, and stops. It may continue to beat for some minutes after the respiration has ceased.

As to the explanation of these symptoms, or, in other words, as to the real factor which comes into play in compression of the brain, it is quite clear that in some cases at least the actual compression of the brain is not the main factor in the production of the symptoms, that there are associated changes in the brain itself, and that some of the symptoms are dependent on laceration of the brain, and so on. This is proved in cases of depressed fracture of the skull by the fact that after you elevate the fragments the symptoms continue; therefore there must be some added condition. That added condition is usually hæmorrhage from the surface of the brain, either occurring independently of the fact of compression or occurring as a consequence of the fact of depression. Now, since the skull is a cavity with absolutely unyielding walls, it follows that if it is depressed the depressed bone or tumour, or blood-clot, or anything which is there, must occupy its position at the expense of the fluids in the skull. It does so first at the expense of cerebro-spinal fluid, and secondarily at the expense of the blood. If the compressing agent be but slight, that is to say, if there is not much to be compensated for, the cerebro-spinal fluid in the lateral ventricles passes to the fourth ventricle, and escapes from the fourth ventricle to the sub-arachnoid space in the spinal canal, and very soon, in consequence of this, and in consequence of diminished exudation from the vessels, and an alteration in the circulation generally, the matter is put right, and the patient's compression is practically compensated for. On the other hand, if the compressing agent be pretty large the amount of compression that it produces is not compensated for, either by any alteration in the transudation from the vessels, or

by any alteration in the circulation itself. And since the cerebro-spinal fluid enters the fourth ventricle faster than it can escape from it, it must follow that the antero-ventricular tension must be raised; the effect will be to compress the capillaries; in fact, that would be the case were it the fourth ventricle or any other part. As a consequence of increased ventricular pressure you must get general cerebral anæmia. If that is the case it naturally follows that the function of the portion of the brain which is rendered anæmic cannot be properly carried out, and we know that in the floor of the fourth ventricle some of the most important vital centres are situated. There are three which we are specially concerned with, namely, vaso-motor, cardio-inhibitory, and respiratory centres. There is also the centre for deglutition. The respiratory centre is the most sensitive of all of them; consequently that first responds to the condition of general anæmia. The respiration becomes slow and deep, and it does so in proportion to the profundity of the anæmia. If the case is going to end fatally the respiration stops before the heart. The cardio-inhibitory centre is affected, and as a result the pulse becomes slow and feeble, and with the affection of the vaso-motor centre the pulse becomes full. The deglutition centre is also affected injuriously, but it is not of so much importance as the affection of the centres upon which a man's very life depends.

If the compressing agent is so situated, and of such a nature, that you can relieve it, the first thing that happens is that the brain can again expand, the lateral ventricles can again contain cerebro-spinal fluid, and thus the tension within the fourth ventricle is diminished. Thus tension being removed, the capillaries are no longer compressed, matters rapidly return to normal, and the patient gradually recovers.

Of course there is yet a good deal to be learnt about the actual pathology of compression of the brain, but at the same time the explanations which I have given you are at any rate consonant with what we observe clinically, and they do serve to point to the fact that one method of dealing with certain cases of compression of the brain is by draining the sub-arachnoid space. You may be aware that in cases of tuberculous meningitis which are slowly passing on to compression, it is now a recognised surgical procedure to trephine

the skull, and tap the sub-arachnoid space at the tentorium cerebri. This I have done on more than one occasion, and certainly have relieved the patient by it very considerably. By this means one draws off the superabundant fluid, and though your compressing agent remains the same, you have provided an outlet for the cerebro-spinal fluid, and so to some extent compensation is set up.

With regard to the prognosis of compression, this necessarily varies with the cause. In compression due simply to depressed bone, the mere mechanical action of the bone upon the brain, without any associated damage, the prognosis is excellent. You lift up the fragments, and so relieve the compression, keep the wound aseptic, and the patient recovers. Compression that is due to the pressure of blood-clot is least hopeful when that blood-clot is dependent upon sanguinous apoplexy, and it is almost hopeless if it is dependent upon an extensive laceration of the brain, which virtually comes to the same thing; it is only a question of how the vessels were ruptured, that is to say, whether they rupture spontaneously, or whether they rupture by injury. Compression dependent upon hæmorrhage from the middle meningeal artery is hopeful in some cases, but absolutely hopeless in others; the prospect depends entirely upon the associated damage, which can be gauged to a certain extent by the time at which the symptoms make their appearance, and by the severity of those symptoms when they do appear. Mr. Jacobson, who has written what in my judgment, is the finest article on middle meningeal hæmorrhage that there is, classifies the cases into practically three categories. There is one category in which he classes the middle meningeal hæmorrhage which is but slight in amount, is absolutely uncomplicated with damage to the brain, and only with perhaps a small fracture in the skull, in which the symptoms make their appearance late, and are not very severe, and in which trephining is early resorted to. Those are the most hopeful according to his selection of cases. There are others in which there is an extensive damage to the skull, perhaps fracture of the base, and fracture of the vertex as well, and perhaps severe concussion at the time of the injury, and associated with severe laceration of the brain with all its symptoms. These are virtually hopeless, and in them trephining is of no use. Then there is a

third class of case which is intermediary between the two I have mentioned, and in these the prognosis is very very bad. In fact, of all the cases of middle meningeal hæmorrhage which I have seen or treated myself, and I have trephined a good many, I have never seen a patient saved. I have trephined four during the last year, and although in every one of them there was a temporary amelioration of symptoms, they all died. The truth is that the class of cases which Jacobson regards, and which everybody else would regard, as the most hopeful, are much the rarest of occurrence. To get a man with sufficient damage to tear his middle meningeal artery, but without other associated injury to his brain or skull, is a very rare occurrence. Though that is true, there are still many cases where one feels justified in trephining; and there are other cases where I should not feel justified in trephining. I remember we had one here eighteen months ago in which I declined to trephine. I was sent for at night, and though the man had middle meningeal hæmorrhage I absolutely declined to trephine, because it was clear to me that he had associated damage, and that if I had trephined him I should not have done him any good, and to do an operation merely for the sake of doing it, without any real hope of benefiting the patient, is not surgery; such a course, it seems to me, would bring surgery into disrepute. The specimen from that case is in the museum, and some of you may remember it.

As to the treatment of compression, it may be dependent on the growth of a tumour, and then one must be guided entirely by the particular circumstances of the case. If the tumour is evidently a growing tumour, and presumably situated in the cortex, and the patient be otherwise in good health, trephining is absolutely and clearly indicated. Yet at the same time it must not be forgotten that in cases of trephining for cerebral tumour the percentage of cases in which it is even justifiable is exceedingly small, something between seven to nine per cent; some observers put the number even lower than that. Again, it must not be forgotten that the mere fact of trephining a patient may cause a scar in the brain which will leave his condition as bad as before, even if the tumour itself does not recur.

In cases of compression from tubercle, as a general rule trephining is useless, for the simple

reason that tubercle is hardly ever present as an isolated mass, and there may be several masses at various parts. You may only have diagnosed one; you may operate and remove that mass, but yet the others will remain, and your patient will die of tuberculosis of the brain just the same.

In cases of gumma of the brain, as a rule trephining is not necessary, because the gumma will clear up under anti-syphilitic remedies, despite what some people say to the contrary. If after trying anti-syphilitic remedies in good doses for some time the symptoms do not disappear, the inference is that you are not dealing with gumma at all. Then, of course, you must trephine according to the general condition of the patient and the localising circumstances.

In trephining in cases of inflammatory trouble, such, for example, as tuberculous meningitis, or meningitis from ear disease, or abscess, you must again be guided by circumstances. I need hardly say that no surgeon would let a patient die of compression from cerebral abscess if he had the means at hand for trephining, because to leave a cerebral abscess is absolutely certain death, but to open it gives a man a very excellent chance of getting well. In tuberculous meningitis you may benefit the patient to some extent by trephining and opening up the sub-arachnoid space. But it must be remembered that compression is not the chief feature of the disease, it is merely an accident in its course, and is one of the ways in which nature terminates a condition of things which is practically fatal in itself.

In trephining for middle meningeal hæmorrhage, which is the main factor which we should consider, the rule is to put your trephine at a distance of from one and a half to two and a half inches from the external angular process, and the same distance above the zygoma. Whatever distance you go behind the external angular process, you must go above the zygoma. The reason that any point between those two limits will do is because the artery runs a very tortuous course. The best plan, and one which I follow, is to take two inches from these situations. When trephining for the middle meningeal hæmorrhage, directly you have separated the periosteum from the skull you should look for a fissure in the bone. That fissure will show itself as a very slight elevation, which you can feel with your finger-nail, along which blood

will be welling up. If you see such a fissure in the bone it is, to a certain extent, confirmatory evidence in favour of your diagnosis of ruptured middle meningeal artery. But if you do not see it this by no means negatives the diagnosis, because, as I said before, you may have laceration of the middle meningeal with only fracture of the internal table, or it may occur when there is no fracture at all, but the dura mater has been simply shaken off the bone, the artery being torn in the process. For removing your ring of bone you should use about an inch trephine, and when you have taken out the bone you will find, if your diagnosis be correct, that blood immediately wells up into the wound, or the bottom of the wound is full of blood-clot, and probably the bleeding will still be going on.

If the bleeding has stopped when you expose the clot, my advice is to take a blunt spoon and take away a *little* of the clot. Do not remove it all, but just enough to relieve the pressure; the rest will become absorbed or consolidated, and you can take it out at a subsequent time if you think fit. If you try to take much of it away you will probably start the bleeding again. The removal of a certain amount of clot will probably relieve the compression, and that is all you want.

Should, however, you find your vessel is still bleeding, then the obvious thing is to attempt to get hold of the vessel. If the vessel is lying in the groove in the bone, or in a canal in the bone, this is simple enough; all you have to do then is to plug the canal with a little sterilised wax, or if you prefer it with sterilised silk or catgut. Unfortunately this is not the common condition. The most common event is for your artery to be lying at the bottom of a deep hole, and between it and you there is a lot of blood-clot. You must get your blood-clot out, and you must try and find the bleeding spot. This may be easy, or it may be difficult. You may use a sponge to keep pressure on the bleeding spot, and when you take it away find that it starts bleeding again. If so, try and pick the artery up, and if you can do so try and ligature it. That is difficult, because your blood-clot is in the way. If you cannot pick it up, but you can still see where it is, take a thread and a very small needle and pass it through the dura mater on one side of the artery, and up through the dura mater on the other side of the artery, and tie the whole thing in together.

There are a certain number of cases where you cannot get at the artery in this way, and then comes the question, should you follow the ordinary rule of treatment in primary hæmorrhage, or should you under the special circumstances of the case adopt some other method? The ordinary rule of treatment of primary hæmorrhage is this: catch your vessel at the bleeding spot if you can, and if you cannot, enlarge your wound until you can get it, and failing to do that, tie the artery in its continuity. If you are going to follow the ordinary rule in cases of middle meningeal hæmorrhage it is obvious you must chisel away more bone, but even doing this will very often not enable you to catch the artery. At the same time I think the proper course would be to do that, provided under the circumstances of the case it seemed feasible. I do not think you can lay down any law in the abstract as to what you should do. If I thought it was possible to pick up an artery by cutting a little more bone away I should do so.

If this fails you are driven to tie either the common or the external carotid, or to adopt some other means. If you tie the common carotid you are not only cutting off the blood-stream through the middle meningeal artery, which is the one in fault, but you are cutting off the blood-stream from that side of the brain as well. That is very dangerous. The brain has probably already suffered some damage, and to cut off its blood-supply is to court disaster. At the same time there can be no doubt that ligature of the external carotid is not always successful. The circulation through the meningeal arteries and the anastomosis between the meningeal arteries is very profuse, and the mere fact of cutting off the blood-supply from one meningeal may be followed by recurrent hæmorrhage when the circulation is fully established again. So that even this has its disadvantages.

One of the best plans you can follow before you resort to ligaturing in continuity is to make a freezing mixture of ice and salt and put it over the side of the man's head and neck, as far as possible, and prop him up in a more or less upright position. In two cases I have seen this stop the artery bleeding pretty quickly, and that is what I should do. If I found I could not tie the artery at the spot, and if I thought that cutting away more bone would probably not enable me to reach it, I should

put this freezing mixture over the head and neck, and reserve the ligaturing in continuity as a last resort.

It has also been suggested that if you can see the artery at the bottom of the wound and cannot tie it, you can touch it with a fine point of a cautery which is heated to a black heat. This might at first appear to be a very risky and dangerous proceeding in view of the presence of the dura mater and the pia-arachnoid, and the brain under that, but as a matter of actual fact, I believe it has never yet produced any harm. If you read Mr. Jacobson's article in, I think, 1884, in 'Guy's Hospital Reports,' you will find that all the things that I have told you of have been tried, and all successfully. But do not run away with the notion that any one of these plans is universally applicable to all these cases. And do not leave with the idea that it is merely a question of hæmorrhage, and that the checking of that hæmorrhage is an easy job, because I would repeat the statement I made just now, that all the cases of middle meningeal hæmorrhage which I have had have died—every one of them.

Pregnancy and Normal Delivery One and a Half Years after removal of both Ovaries.—

Kossmann reports the following unique case:—On November 12th, 1895, he extirpated both ovaries in a woman who suffered from oöphoritis. The tubes were not removed. Vagino-fixation completed the operation. About one and a half years later the husband reported his wife pregnant, which was verified at a subsequent examination. December 2nd Kossmann was summoned to patient, but upon arrival (two and a half hours after beginning of labour) found her delivered of a living child. It must be surmised that particles of ovarian tissue were left behind, which remained active. At the time of this report patient was again pregnant.—*American Journ. Obstet.*, June, 1900.

Suppository for hæmorrhoids:

Chrysarobin	1 gr.
Iodoform	$\frac{1}{2}$ gr.
Extract of belladonna	$\frac{1}{8}$ gr.
Cocoa butter	30 grs.

Therapeutic Gazette, June 15th.

THE DIAGNOSIS AND TREATMENT OF CHRONIC SUPPURATION OF THE MIDDLE EAR.

By A. S. COBBLEDICK, M.B., B.S.Lond.,
Late Senior R.M.O. at the Throat, Nose, and Ear Hospital,
Golden Square.

THE following remarks are applicable to those cases of middle ear disease which persist for many years, with occasional intervals of freedom from discharge, and which, in spite of the ordinary routine treatment, regularly and carefully carried out, do not really improve, and continue to cause annoyance to both the patient and medical adviser. Many patients so afflicted in time cease to regard their condition as at all dangerous, and allow the trouble to take its chance, until some acute and not uncommonly serious complication causes them to again seek medical aid. Especially is this the case with those whose hearing power is not much affected from the disease being unilateral, and in whom attacks of pain are infrequent or wholly absent.

Every attempt should be made by the medical man to stop the disease, and if he does not succeed he should advise consultation with an otologist. When it is fully realised how many of these cases terminate with cranial complications, entailing a severe major operation, not infrequently with a fatal result, there can be no question as to the medical man's responsibility and duty.

Much can be done in some of these intractable cases short of a radical operation, and not infrequently with a most successful result, both as regards a diminution of the discharge and a distinct improvement in the patient's hearing power.

The first step to be taken in these cases is to make a thorough examination of the external meatus and middle ear, and to diagnose from the appearances and symptoms as exactly as possible the pathological condition causing the trouble.

The condition of the middle ear may be obscured by profuse purulent discharge and considerable swelling of the meatal walls with a severe eczematous condition of the outer part of the meatus.

Careful treatment, described later, must be carried out to remedy this condition. The possibility of the presence of a foreign body should always be borne in mind, as they are readily overlooked in old-standing cases with constriction of the meatal canal. The following case, which ended fatally, forcibly impresses this point. A man aged forty-seven came to the out-patients' with an offensive discharge from the right ear, which had troubled him from childhood. He had a very swollen and almost occluded meatus, with a small polypus apparently springing from the posterior wall close to the constriction. Temperature 101° F. Great pain over the right temple and forehead, but no swelling or pain on pressure over the right mastoid region. General condition very bad.

On snaring the polypus, to get a better view of the deep parts, a few shreds of wool were caught in the snare, a plug of wool was searched for, and after much difficulty a large piece was extracted with forceps. Its removal was followed by a flow of about two drachms of dark grumous semi-liquid and intensely offensive pus. A radical operation was performed; a large piece of necrosed bone, forming the roof of the attic, was discovered, and upon further opening up the middle cranial fossa a temporo-sphenoidal abscess was discovered. The patient died on the eighth day after the operation.

The patient or friends could give no information as to how long the wool had been in the ear.

An abnormally narrow and tortuous meatus may cause much trouble, and very frequently a good view of the parts cannot be obtained without causing the patient some pain.

There is only one point about the character of the discharge in these cases which is of much practical value, and that is the presence of particles of cholesteatoma. If this substance is found in the washings from a suppurating ear it influences the prognosis and points to a definite treatment.

The only other common condition that obscures a view of the tympanum is the presence of aural polypi. In these intractable cases of middle ear suppuration they are large and frequently recurrent. This recurrence is due to the point of origin of the polypi being too deeply seated to be reached by intra-aural methods.

When the meatus has been cleared of all *débris* and obstructions, the condition of the membrane and ossicles should be carefully noted. Most of

these suppurative cases which have for years withstood ordinary treatment show considerable destruction of the tympanic membrane and malleus, which may be so extreme as to expose the whole of the inner wall of the tympanum. In another class of case the tympanic portion of the membrane may be almost intact, but on careful examination a large perforation is seen high up in Shrapnell's membrane; this condition especially indicates the presence of either caries of the head of the malleus, cholesteatoma, or both combined, as the exciting cause of the discharge.

All degrees of destruction are met with; the handle of the malleus may be only foreshortened, or it may have wholly disappeared, only leaving the processus brevis to indicate any remains of the malleus; this latter process serves as an excellent landmark in making out the anatomy of the parts.

Extensive destruction accompanied with discharge points to either—

1. Caries of the ossicles.
2. Caries of the walls of the attic.
3. Cholesteatoma or chronic suppuration in the mastoid antrum.

It is often most difficult to say how much deep destruction there is as a result of the caries, and, as will be shown later, it can often only be estimated by a process of exclusion in the treatment.

The presence of cholesteatomatous particles in the discharge makes the diagnosis clear, but the amount of accompanying caries is not always evident.

Of the two classes of cases, I should say that cholesteatoma is more frequently associated with destruction of Shrapnell's membrane and caries of the head of the malleus, than with perforation in the tympanic membrane, and foreshortening of the malleus.

There is another not uncommon type of case in which, during an interval of quiescence, cicatrices or adhesions have formed between the membrane or hammer and the promontory of the tympanum. The result is, that when a relapse occurs, the free flow of pus is seriously retarded, and not infrequently gives rise to considerable pain and somewhat severe general symptoms.

In another class of case there is a large perforation in front or behind the handle of the malleus, with extreme thickening and redness of the remaining membrane, so that the malleus handle is not distinguishable.

There is a very troublesome type of middle ear suppuration characterised by very severe attacks of pain rather than by profuse discharge or destruction of middle ear contents. The history of these cases ranges over many years; the pain is not due to pressure caused by retention of discharges, nor is it relieved by the removal of the ossicles; it is due to a sclerosis of the mastoid process, and can only be relieved by freely opening the antrum, and removing the superficial sclerosed bone.

In all these cases the hearing by the conducting media is severely impaired; the process, however, seldom extends to the internal ear, and it is surprising how uniformly good the bone conduction is in these old-standing cases; in this respect they form a great contrast to the catarrhal middle ear affections.

Pain is frequently completely absent, and in part, no doubt, accounts for the patient's callousness of his condition. Apart from the pain caused by severe complications, any that occurs is usually paroxysmal, and is due to the temporary retention of the discharge; this is most commonly due to obstruction caused either by—

1. Foreign body.
2. A mass of cholesteatoma.
3. Large masses of polypi.

Treatment.—The treatment is entirely based upon the condition of things diagnosed in the meatus and middle ear.

Those old-standing cases of suppuration with marked swelling of the meatal walls, and an eczematous condition of the outer ear, require careful daily treatment for a week or ten days. After cleansing the parts with warm boracic lotion, the meatus should be packed with a strip of cyanide gauze soaked in a lead and tar or lead and alum lotion; I have found the following lotion very useful for this purpose:

℞ Liq. plumbi subacet.	} āā 3ss.
Liq. carbonis deliq.	
Glycerini	3j.
Aquam ad	3j.

The packing should never be left in for more than twenty-four hours; at the end of such time it should be removed, the ear cleansed with boracic lotion, and the meatus again plugged. If this is followed out for some days a very great improvement takes place, the eczematous condition improves, the meatus becomes dilated, and a thorough

examination of the middle ear is rendered possible.

The treatment of the other condition which may obscure the examination of the middle ear, viz. polypi, must be careful and thorough.

As they are usually bathed in a foul pus it is not wise to remove them indiscriminately and without preparation. The best method of procedure is to have the patient under constant supervision for a few days previous to operation, during which time the meatus may be kept clean by frequent irrigations with warm boracic lotion, followed by the instillation of rectified spirit. After three days of such treatment, the polypi may be removed under a general anæsthetic, preferably chloroform or A.C.E.; the venous congestion produced by ether anæsthesia increases the bleeding which is at any time a troublesome factor in the removal of polypi.

Aural polypi are very vascular, and the chief trouble in the operation is that, after the greater part of them has been removed with the aural snare, further proceedings are quite obscured by the bleeding.

This can largely be obviated by the use of equal parts of a 20 per cent. cocaine solution and 50 per cent. solution of the extract of supra renal capsule: I cannot speak too highly of the latter drug in controlling the bleeding in the operation. The above mixture should be dropped into the meatus about ten minutes before commencing the operation; as much of the solution as possible should be removed before the polypi are snared, by using dry mops of absorbent wool, for the possibility of a considerable quantity of the 20 per cent. cocaine solution reaching the middle ear and passing down the Eustachian tube, must be borne in mind.

The greater part of the polypus can be removed without difficulty by means of a Wilde's aural snare. By careful mopping and inspection, the condition of the membrane, if it has not been already destroyed, may be made out. The remains of polypi and granulations must be removed with a small ring or spoon curette, until the inner wall of the tympanum can be plainly discerned; the curetting should not be forcible, as the possibility of the facial nerve being exposed on the inner wall must be borne in mind: this operation has not unfrequently been followed by complete facial paralysis.

If the polypi have recurred after previous re-

moval, it is probable that the membrane and ossicles have been destroyed by the disease, or removed at a previous operation, and that the polypi have origin around a sequestrum in the region of the attic.

For the removal of granulations and remains of ossicles in the attic, Lake's attic curette is a most useful instrument.

The parts should be well cleansed with one in sixty carbolic lotion, after the completion of the operation, thoroughly dried and firmly packed with a long strip of iodoform gauze, moistened with iodoform emulsion. The emulsion renders the removal of the plug much less painful than if dry gauze is used. The packing need not be removed for forty-eight hours.

Untoward results of this operation are:

1. Facial nerve paralysis.
2. Labyrinthine vertigo which may persist from three to ten days.

There now remain those cases of persistent suppuration, unaccompanied by growth of polypus, where removal of the ossicle is indicated; it is in these cases, when properly selected, that such excellent results are obtained.

The following are the main indications for this operation:

1. Obstinate middle ear suppuration which resists all less radical forms of treatment.
2. In cases where the flow of pus from the upper tympanic spaces is retarded, through the handle of the malleus and central part of the membrane having become adherent to the inner wall of the tympanum.
3. Cholesteatoma in the upper tympanic spaces or mastoid antrum; in both these conditions the ossicles are usually carious, and present a barrier to the free exit of pus and cholesteatomatous masses.
4. Cases of suppuration in the external attic with perforation in Shrapnell's membrane, provided the hearing power is very much impaired.
5. If middle ear suppuration has been in abeyance for a considerable time, and the resulting cicatrices have produced severe disturbance of hearing, and subjective symptoms of dizziness and fulness of that side of the head from increased labyrinthine pressure, the removal of the remains of ossicles is strongly indicated.

Operation for the removal of ossicles.—This can

very frequently be carried out under cocaine anæsthesia; the following is a rough test as to whether the operation is likely to be tolerated without an anæsthetic: if the parts can be examined with a probe without causing the patient to flinch, cocaine is usually sufficient, but if the parts are extremely sensitive to the touch of a probe, no amount of cocaine seems to produce a satisfactory anæsthesia.

Highly nervous women usually require general anæsthesia.

The best method of producing local cocaine anæsthesia for this operation is to soak a small pledget of absorbent wool in a 20 per cent. cocaine solution, and cover the moist surface with some powdered cocaine. Press the wool firmly against the remains of the membrana tympani, and leave it there from ten to fifteen minutes. This is a much better proceeding than pouring a 20 per cent. cocaine solution into the affected ear until the meatus is filled. The last method is open to the objection that a considerable quantity may pass down the Eustachian tube to the pharynx and be swallowed. In one case where this method was adopted I noted severe toxic symptoms of the drug, viz. palpitation and tachycardia, dilated pupils, and a fair amount of collapse, necessitating the free use of stimulants and the postponement of the operation for thirty-six hours.

The operation, as frequently described, requires a special set of instruments, but it can be carried out quite as efficiently with such as are contained in all aural cases. These are a paracentesis knife, small tenotomy knife, ring curette, fine angular forceps, and incus hook.

If the lower half of the membrane has been destroyed, and the handle of the malleus in part or wholly remains, it should be separated from the membrane by two incisions, one in front and one behind the handle, commencing high up and passing down to the lower limit of the membrane. The handle is next freed from the surrounding parts by division of the tensor tympani muscle, and any adhesions between it and the inner wall of the tympanum by means of the curved tenotomy knife. The tip of the free handle should be threaded into the small ring curette, and firm upward pressure exerted, so as to dislocate the joint between the head of the malleus and the incus. After this proceeding there is usually no

trouble in picking out the malleus with fine forceps or a fine sharp aural hook.

The incus is frequently much more difficult to remove, and may not be extracted after careful and repeated attempts. I think the reason of its mysterious disappearance is frequently due to the fact that at the time the malleus is dislocated the incus is also forced downwards or backwards from its normal position.

If it remains in position, and has not been largely destroyed by caries, it may be extracted by passing the incus hook into the posterior part of the attic space and rotating it downwards and forwards. In this way the hook passes on to the upper surface of the incus and forces it downwards into the tympanic space.

The stapes is difficult to remove. I have seen it removed accidentally with no good or bad result. On attempting to remove it in one case I produced a most severe labyrinthine vertigo, which persisted for six days. The indications for removal of the stapes are not very clear, and the results obtained have varied considerably.

I may say that practically in the treatment of chronic suppuration of the middle ear the stapes may be ignored.

This operation, as above described, has to be modified according to the amount of destruction that the membrane and ossicles have undergone.

In cases where there is a large perforation in Shrapnell's membrane, and the removal of the ossicle is indicated, the whole of the tympanic membrane should be removed by making a circular incision close to its peripheral attachment.

As in the case of curetting the middle ear for the removal of polypi, so facial paralysis and severe vertigo may follow the removal of the ossicles.

A brief description of a few cases, with the result some months later, will be of interest.

Case 1.—Male, aged thirty-two, with purulent discharge from the left ear since the age of ten years. Large perforation present, involving the lower half of the tympanic membrane, and some foreshortening of the malleus handle. Watch not heard when held close to the ear; bone conduction good.

December 30th, 1899.—The left malleus was removed under cocaine. Immediately after the operation the watch was heard at a distance of six inches. A month later the patient returned. The

watch was heard well at four inches, and the discharge had completely disappeared. Two months later he described himself as cured; no discharge, and "quite clear in the head." Improvement in hearing power maintained.

Case 2.—Male, æt. 24, with almost continuous discharge from the left ear, in spite of treatment, since the age of eleven years. Large perforation in the anterior inferior segment of the membrane, with thickening of the membrane around the malleus, which was adherent to the promontory. Watch heard at three inches.

November 29th, 1899.—Malleus removed under cocaine—carious malleus.

February 5th, 1900.—No discharge since the operation. Watch heard at five inches.

February 14th, 1900.—Watch heard at eight inches. Patient very pleased with result.

Case 3.—Female, æt. 32, complained of running from the left ear for the past ten years. Treatment had had but little effect. Of late she has suffered from tinnitus—severe—mastoid pain and occasional attacks of vertigo. Most of the tympanic membrane and handle of malleus destroyed. Watch not heard on contact with lobule of ear.

December 17th, 1899.—The patient developed toxic symptoms of cocaine, and the operation had to be postponed.

December 19th, 1899.—The malleus and incus were removed under chloroform; both ossicles were considerably eroded.

December 26th, 1899.—Patient states that the tinnitus has almost completely disappeared.

February 7th, 1900.—Slight tinnitus and pain at times, but the head is very much clearer. Very slight discharge. Watch heard at two and a half inches.

Case 4.—Female, æt. 30. Running from the left ear since childhood, with short intervals of freedom. Of late she has complained of tinnitus and fulness in the head, with increased discharge. Remains of handle of malleus adherent to inner wall of tympanum; most of membrane destroyed. Watch only heard on contact with lobule.

January 2nd, 1900.—Malleus and incus removed under cocaine.

February 2nd, 1900.—Tinnitus has completely disappeared. Watch heard at two and a half inches.

March 4th, 1900.—Very slight discharge. Watch

heard at four inches. Patient has very much improved.

These cases show how the discharge can be lessened or cured, and subjective symptoms and hearing power distinctly improved by this operation. Of course there are a certain number of cases which are not as satisfactory, *e.g.*

Case 5.—Male, æt. 29, who had suffered from discharge from the right ear ever since childhood. Of late he had had occasional attacks of severe pain and vertigo, relieved by a free flow of pus. Patient extremely deaf. There was a large perforation in Shrapnell's membrane, through which the attic could be explored and irrigated. Examination of washings showed the presence of cholesteatoma. The ossicles were removed under chloroform, and the attic curetted. The head of the malleus was almost entirely destroyed, and the incus was superficially eroded. When seen six weeks after the operation the patient's subjective symptoms were better, but the discharge of pus, if anything, was more copious.

This is undoubtedly a case of cholesteatoma in the mastoid antrum, and nothing short of a radical mastoid operation will effect a cure.

Therefore cases which do not show a very considerable improvement after three or four months of careful intra-aural treatment will probably require a radical operation. With regard to the exact indications for the complete operation, it is very difficult to lay down hard and fast rules. Generally speaking, the subjective symptoms of dizziness, tinnitus, and fulness in the head are so improved by the removal of the ossicles that they do not need to be considered under this heading. If, however, they do persist, and are associated with pain and purulent discharge, the combination is a very strong indication for the complete operation. Pain alone, whether paroxysmal or continuous, is a strong indication, especially if the cause can be ascribed to pressure of an increasing mass of cholesteatoma. Recurrent polypi point to a deeply placed sequestrum, and is yet another indication. Any of these conditions in an acute form should be regarded as urgent, and in very few cases does any advantage result from postponing the operation. Radical operation should not be pressed for purulent discharge only, *i.e.* unassociated with any subjective symptoms, more especially if the hearing power is only slightly

impaired. It is not here necessary to describe the operation in detail, but I may say that there is only one operation that can make these cases quite safe, and that is a radical mastoid operation. The antrum should be opened, the posterior bony meatal wall removed, and the attic, tympanum, and antrum thrown into one cavity, so that the whole can be readily packed. These cases should be packed from the external meatus, so that the wound may be closed and healed within ten days time from the operation. To facilitate packing through the external meatus Panzy's flaps should be shaped from the cartilaginous meatus by slitting up its posterior wall with a scalpel, from its inner extremity to the concha; before closing the wound one flap should be anchored upwards and one downwards by deep silk sutures.

By the above means the whole cavity comprised of tympanum, attic, and antrum can be packed from the external meatus efficiently, and without causing the patient much pain; at the same time the wound is allowed to heal, and treatment of a troublesome sinus behind the ear is not necessary.

Summary of Treatment.—1. Removal of all obstructions in external auditory meatus, whether due to foreign body, constriction, or polypi.

2. Removal of malleus, or malleus and incus, and curettement of the attic space.

3. If this is not followed by a great improvement in three or four months, the mastoid antrum should be opened, and search made for a sequestrum or cholesteatoma.

Impetigo: Treatment.—Kistler ('Münch. Med. Woch., May 1st, 1900, p. 634) recommends the following application for the treatment of impetigo of the scalp and face in children:

R. Acid. salicyl.	2 grms.
Bism. subnitr.	40 "
Talc. pulv.	15 "
Ung. aquæ rosæ	100 "
M. F. pasta.			

Medical Age, July 10th, 1900.

THE RESPIRATORY AFTER-EFFECTS FOLLOWING THE INHALATION OF ETHER.

A discussion at a Meeting of the Society of
Anæsthetists.

The President, Dr. J. FREDK. W. SILK, in the
Chair.

(Continued from p. 255.)

Mr. EASTES said he had very few facts to give the Society; but from some statistics upon which he had been engaged for some time in conjunction with Dr. Dudley Buxton and other anæsthetists it appeared that pneumonia was extremely rare after any anæsthetic, there being only four instances of pneumonia in 20,000 to 30,000 cases. In that series of cases pneumonia had not occurred more frequently after ether than after chloroform anæsthesia. With regard to bronchitis, it was certainly less rare than pneumonia, but the average after cases of chloroform anæsthesia was only one in 3,300; whilst after ether it occurred once in about 400 cases, which was very parallel with the statistics quoted by Dr. Dudley Buxton, who, he thought, said he had one case of bronchitis in every 423 cases. Curiously, he had had, in his own practice, no cases of ether administration followed by either any bronchitis or pneumonia, but he would like to mention two cases which had occurred after chloroform. They were not very cognate to the discussion, but he would give them briefly because they were interesting. One was a case of carcinoma of the floor of the mouth in a gentleman, who was operated upon by a London surgeon. He, Mr. Eastes, did not administer the anæsthetic, but was present at the operation, which lasted a considerable time. Chloroform was the drug given. The throat was being constantly swabbed out, and he had no doubt that during the operation some of the septic discharges from the carcinomatous growth got at the back of the mouth and were sucked into the lung. On the second or third day pneumonia developed and the patient

had a serious illness from which he nearly died. The other case was one of bronchitis which occurred in a very curious way. It happened at Guy's thirty years ago, when he was surgical registrar, and was in a case of tetanus which was under the care of Mr. Cooper Foster. The patient was a boy of seventeen or eighteen years of age, and Mr. Cooper Foster was very anxious that the patient should be kept under chloroform day after day by relays of students; which was done. In that way the spasms were mitigated, the patient being allowed every now and then to come to sufficiently to enable him to take food enough to carry him on. That went on for nearly four days, during which time the tetanic spasms were diminished and were hardly visible at all. The patient seemed really to have been cured of his tetanus, when unfortunately very extensive bronchitis developed, of which he died. In that case the malady was verified post mortem.

Mr. BURTON said he had much pleasure in giving his experience with regard to the administration of ether and its results; but with regard to subsequent pulmonary trouble, he only remembered one case of many thousands—certainly 4,000—in which he had seen anything like either bronchial catarrh or any other pulmonary affection. In that case ether was given to a lad of about seventeen years of age for a very slight operation—the removal of a finger. The boy was under the influence of the anæsthetic altogether for about twenty-five minutes, and thereafter he developed a pretty smart attack of bronchial catarrh. The ether was not given in considerable amount, and it required but little to keep him under. But it transpired afterwards that at the time there was a “cold going through the house.” On the other hand, with regard to chloroform administration, he had had a very considerable experience of that, and had seen very many cases of bronchial catarrh and pneumonia following its administration. But he did not in the least attribute those attacks to the effect of the chloroform vapour upon the lining membrane of the bronchial tubes or cells. He quite agreed with what Dr. Dudley Buxton had said with regard to the causation of those attacks—that they were chiefly due to the exposure of the patient, along with the devitalisation partly due to shock of the operation and partly to the expo-

sure to cold. There was also devitalisation produced by the anæsthetic itself. He remembered very distinctly two cases which had occurred in his practice quite recently and in each of which chloroform exclusively was used at an anæsthetic; in one of these the operating surgeon had a very severe attack of coryza, so much so that it seemed he would hardly be able to finish the operation, which was amputation of the breast. The nurse, for obvious reasons, could not help him. He dared not wipe his nose himself, and it therefore fell to the lot of the anæsthetist to perform that function. In that case the anæsthetist had not the opportunity of cleansing his hands, and an attack of bronchitis developed in the patient, but was not of severe degree. The other case was one in which the nurse was suffering from coryza, and she was constantly having to disinfect her hands and return to the case. In that case also the patient developed an attack of bronchitis. Apropos of that, he thought it would be a very important point to examine rapidly but in a routine manner, every patient who was going to have ether, when no doubt it would often be found that laryngitis or coryza or some bronchial trouble preceded the administration of ether. It would also be advisable to ascertain whether there was “a cold running through the house.” It is the modern belief that ordinary coryza is an infectious disease, of which the ætiological factors are (a) some devitalising cause or causes, and (b) exposure, during the influence of the latter, to catarrhal infection. Now devitalising causes are present in abundance during the administration of chloroform or of ether for a surgical operation; there are:—(1) Fright; (2) Hunger; (3) Thirst; (4) Exposure of skin areas (usually protected by clothing) for, sometimes, one hour, two hours, or longer; (5) Loss of blood; (6) Shock due to operation; (7) Depressing effect of the anæsthetic—more especially of chloroform; while, as for exposure of the patient to catarrhal infection under the above conditions, such exposure has probably occurred in every case in which bronchitis has developed subsequently to the operation. This probability will be the more readily conceded when it is remembered how frequent, under all conditions, is the exposure to catarrhal infection on the one hand and, on the other, how extremely rare it is to find an

attack of bronchitis following ether administration.

Dr. PROBYN-WILLIAMS said he would like to answer Dr. Dudley Buxton's question about the statistics at the London Hospital for the year 1897. The number of cases in which ether alone was given was 2,910, and the cases in which ether was the main anæsthetic, in other words, in which it was preceded by gas or a little A.C.E. mixture, were 440, making a total of 3,350 cases in which ether was really the anæsthetic employed. The cases were examined in the wards for the following two or three days. As Dr. Dudley Buxton had said, Dr. Hewitt had reported two cases in which there were respiratory after-effects. One, a woman æt. 41, who had ether in an Ormsby's inhaler for forty-five minutes, had bronchitis, which appeared on the second day, and was followed by broncho-pneumonia, from which she recovered. The other case was that of a boy of fourteen whose appendix was removed. He had ether by Clover's inhaler, for forty minutes. He secreted large quantities of mucus, and died within thirty-six hours. The cause was certified as acute pulmonary œdema, but no post-mortem examination was allowed. The serious symptoms which caused tracheotomy to be performed in one case occurred during the administration of an anæsthetic, and could not fairly be classed as "after effects," as the patient recovered perfectly from the anæsthetic and had no respiratory trouble afterwards.

Regarding Professor Julliard's statistics showing that he had never met with any cases of bronchitis or broncho-pneumonia following his method of inhalation, he, Dr. Probyn-Williams, was with Prof. Julliard, in Geneva, in 1896; and it was true that the method was the open one, but in every case a hypodermic injection of morphia was given before the ether, which would, he thought, invalidate the statistics.

With regard to Mr. Corner's case of pneumonia, he thought it was most unusual to see, in any of the cases in which trouble followed the administration of ether, that the temperature came down by crisis, as Mr. Corner mentioned. He would like to hear what other speakers said on that point. His own experience was that the temperature varied and came down gradually.

They must all agree with Dr. Dudley Buxton

and other speakers as to the difficulties which were experienced in proving, in a pneumonia or broncho-pneumonia following ether, how much the pneumonia was due to the ether and how much to the exposure. But he would like to ask those anæsthetists present who had had a longer experience than he had, whether they remembered, when the carbolic spray was used very much, if the troubles with respiration when ether was given were greater or less than at the present time.

In cases of amputation of the breast, where the chest was bandaged so tightly afterwards, and also in all abdominal operations where the patient has to be kept flat on the back for some days, as a matter of prevention, he entirely agreed with what Mr. Carter Braine had said in his note, namely, that when they knew that the patients would not be able to use their chests to full advantage to get rid of any mucus which might be secreted, it became a question for them to decide, as soon as they saw that the mucus was beginning to be secreted more freely than it should be, whether the anæsthetic should not be changed. He agreed with Dr. Dudley Buxton that whenever possible they should start with gas and ether, but when the secretion showed signs of becoming a trouble to the patient, not only on the operating table but in the following few days in bed, it was a question whether it would not be wiser to change to the A.C.E. mixture or even chloroform.

(To be concluded.)

Invasion through the Tonsils.—Frederick A. Packard, in the 'Philadelphia Medical Journal' of April 28th, 1900, discusses the relation of acute articular rheumatism to the tonsil. He sums up his argument by the statement that the tonsils are active and useful organs, which offer a barrier to entrance of organisms in the deeper tissues, at a point especially open to infection. They act the same as other lymphadenoid tissues, and in this respect are strikingly like lymphatic glands. During the course of or following tonsillitis, there is frequently an attack of acute articular rheumatism. The latter is believed to be an infectious disease, which may be dependent upon more than a single organism. The phenomena of rheumatism can be accounted for by toxic invasion.—*Medicine*, July, 1900.

TRAUMATIC SEPARATION OF EPIPHYSES.

Mr. JOHN POLAND has published an important work on Traumatic Separation of the Epiphyses, and the need for such a book has been emphasised by the growing use of the X-rays in medical work, especially in cases of dispute where the aid of the law is invoked. The whole matter is complicated by the very diverse opinions held, even at the present day, concerning these injuries by men whose names carry great weight, not only with their professional brethren, but also in courts of law. One surgeon's experience is that he detects these injuries very often, while another never does so, or only admits that they are rarely met with. It is interesting to note that Mr. Poland considers this extraordinary discrepancy is due to the absence hitherto of direct examination in a large proportion of cases, and, in part, to the ease with which epiphyseal separation may be confused with dislocation or fracture of the end of the diaphysis. The volume is an exceedingly interesting and readable account of the whole matter of epiphyseal separations from injury, and, although the size of the tome may deter some at first sight, those who open the pages and read Mr. Poland's teaching will find themselves fascinated by the way in which the difficult problems connected with this subject are put forward. To write a book of nearly one thousand pages on a little understood subject in such a manner that the more the reader proceeds the more his interest grows is a veritable triumph, and it is a further matter for congratulation that the author maintains throughout the fifty or more chapters a very high order of literary merit; where condensation has been necessary style is still maintained, and the reader is not disagreeably made aware that his literary fare partakes too much of the nature of so many facts arbitrarily squeezed into so many lines. The value of the publication is much enhanced by the wealth of illustration, which, indeed, the difficulties of points treated renders necessary. It is no easy task to criticise this work, for it is one of those rare productions in these days of haste and hurry that practically disarm adverse comments,

which, if expressed, would and could only apply to trivial matters, and moreover would uselessly divert attention from the real merit of this magnificent volume, and would only disturb the proper perspective under which small matters of detail sink into their real insignificance. The publishers, Messrs. Smith, Elder, and Co., have done their part well, and it is a pleasure to congratulate all who have in any way assisted the author in bringing his valuable teaching before the medical public.

WE have received from the Plasmon Syndicate, Limited, samples of their food preparation termed Plasmon. The substance itself is a dry, white, very soluble, granulated powder, and is the concentrated albumen of pure fresh milk. It is constant in respect to the proportions of its constituent parts, and by reason of its digestibility and nutritive powers is capable of very wide use. The addition of one teaspoonful of this powder to any food increases the value of the food in regard to assimilative power, for one teaspoonful is equal in amount of sustaining value to that of one pint and a half of milk. Inasmuch as Plasmon possesses the virtue of helping digestion it is especially helpful in young and old patients where mal-nutrition and weak digestion are factors of importance. It has been found of great advantage in the treatment of typhoid, and also in nervous cases. Its great value, no doubt, can be attributed to its being palatable, nutritious, and non-irritating, and so easy of administration that it can be sprinkled over food like sugar, or mixed with the flour in bread, or dissolved in soup or milk. When it is remembered that there is often great difficulty in inducing adults to adhere to a milk diet, it will be recognised that in Plasmon, which consists of the proteid of milk free from sugar and fat, the way out of the difficulty is found; it can be confidently expected to afford good results, as it contains such a high proportion of nitrogen. A very practical consideration soon emphasises itself with those who have acquaintance with this food preparation, and that is in relation to the adaptability of this powder in the hands of the family cook when dealing with fastidious patients, and this is easily understood when it is borne in mind that Plasmon retains all the binding properties of albumen. In baking, the cook can add from 10 to 20 per cent. of Plasmon without fear of deteriorating or spoiling the appearance of pastry. It is to be hoped that the cheapness of this preparation will enable it to become a most popular food.

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* Specially reported for The Clinical Journal. Revised by the Author.

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A CLINICAL LECTURE.

Delivered at the London Hospital, March 12th, 1900.

By G. E. HERMAN, M.B., F.R.C.P., F.R.C.S.,
Senior Obstetric Physician to the Hospital.

GENTLEMEN,—I lecture upon two cases to-day: a case of inversion of the uterus by a fibroid, and a case of dermoid cyst of the pelvis. In both of them interest lies in the difficulty in diagnosis.

The first case is that of a patient who complained of hæmorrhage, and applied for treatment to Dr. Waldron, who, having recently held the post of resident accoucheur in this hospital, had had much experience in gynæcological work. Dr. Waldron wrote to Dr. Wilson, our present resident accoucheur, and told him he was sending up a case of fibroid polypus. The history was that she had been regular every month till seven years ago. She had been losing too often since that time. She was at her worst at Christmas, 1899. The fibroid thus appears to have been growing for seven years. The patient said she passed clots. The presence of clot only indicates that the loss is profuse. If the blood is in excess and there is not enough cervical mucus to prevent it clotting, clots lie in the vagina. Three months ago the patient had a sense of heavy weight and bearing down while stooping, which engendered nausea. Six weeks ago she noticed a lump coming down, which was, I suppose, the vagina. In due course the patient arrived, and when I came down the next day Dr. Wilson said to me that Dr. Waldron had sent this case, but that it was not one of polypus as Dr. Waldron had supposed, but one of inversion of the uterus. I was surprised to hear that Dr. Waldron was wrong. (It turned out that both Dr. Waldron and Dr. Wilson were right.) The patient was found to have a tumour bulging through the cervix uteri into the vagina. Both with fibroid polypus of the uterus and with inversion of the uterus, you feel a swelling lying in the vagina and encircled, at its upper part, by the

neck of the uterus. You cannot distinguish between the two conditions by vaginal examination alone.

There are three ways in which the diagnosis may be made. The first, and generally most suitable one, is by bimanual examination, by which, in the case of polypus, the body of the uterus can be distinguished above the cervix uteri, whereas in inversion of the uterus there is no such thing to be felt. But this mode of examination was difficult here because the patient was very fat. Another method is to pull down the cervix uteri with a volsella, and then, by the rectum, to get the finger above the cervix uteri, so as to feel either that the cervix is continuous with the body of the uterus; or to feel the dimple, which, in inversion of the uterus, is where the finger ought to be able to feel the body of the uterus. Dr Wilson had used this method, and had felt this depression where the body of the uterus ought to have been.

A third method is by passing a sound. When the tumour is a polypus of the uterus, the sound will go up by its side through the whole distance to the fundus uteri. If the uterus is inverted the sound will only pass up into the cervix uteri, and then it will be stopped by the reflection of the uterus. This would be sufficient were it not that with a polypus of the uterus you may happen to pass the sound up on the side on which the polypus is attached, and then the sound will not go up. This error can be avoided by passing the sound on the other side of the tumour. I conceive it possible—though I have not known it occur in inversion of the uterus—that the sound might perforate the uterus.

Well, there was this tumour in the vagina, and it was clear also that the body of the uterus was inverted. Inversion of the uterus is produced, so far as we know, in two ways only. One is by the uterus being inverted during the third stage of labour. The common cause of puerperal inversion of the uterus is pulling at the cord. Either the accoucheur pulls at the cord to remove the placenta, which is not often done nowadays, or else the cord is pulled on during the birth of the child; either the cord has been abnormally short, or has been made relatively short by being coiled round the child. As a result there is traction upon it; and this is the most frequent cause of

puerperal inversion of the uterus. It is believed it may occur spontaneously from the patient coughing, or straining, or making some effort, during which the uterus is inverted. This is possible, but it is also possible that in many recorded cases in which inversion of the uterus has been thought to have occurred in that way it may have been produced during the birth of the child by the pulling of the cord which was not noticed at the time. The essential thing for the production of inversion of the uterus is relaxation of the os uteri internum. If the uterus is relaxed, and pressure or pulling makes the fundus uteri come down through the os internum, when the os uteri contracts the body of the uterus will be held down. When puerperal inversion of the uterus is not replaced and the patient survives, the uterus undergoes involution, as it would have done in its natural situation. When involution is complete, at the end of ten weeks, the condition is called chronic inversion of the uterus. This is one way in which chronic inversion of the uterus is produced. The other way is by the traction of a fibroid. The uterus contracts and drives the fibroid through the os internum, and, if then the os uteri contracts, the fibroid is held down, and it can only advance further by pulling the body of the uterus after it, and so it inverts the body of the uterus. There is, in the College of Surgeons' Museum, a preparation by Hunter illustrating this. This is how inversion of the uterus was produced in the present case.

A recent puerperal inversion of the uterus is generally easily reduced by manual taxis. Chronic inversion of the uterus, on the other hand, is difficult to replace by manual taxis. That is why so many of these cases, until about twenty years ago, were treated by amputation of the uterus.

There have been some few operations done to replace the uterus by opening the abdomen, dilating the cervix, and then pushing the body of the uterus back. Those operations were conceived on a sound principle, namely, that it was the contracted cervix which prevented the replacement of the uterus. The reason why few such operations have been done is that when this treatment was proposed antiseptics were not known, and therefore the opening of the peri-

toneum was highly dangerous. About twenty years ago Dr. Aveling introduced his repositor, which reduced the uterus by steady elastic pressure. It consists of a cup which is pressed upwards by india-rubber bands. Pressure is thus continually applied to the body of the uterus for forty-eight hours or more, and in most cases in which this apparatus has been properly used the uterus has been replaced by it. This repositor, although in most cases successful, has not always been so. It may be that the cases in which it failed were those in which there was some fault in its application. That I do not know. One must take the fact as it is, that Aveling's repositor has sometimes failed. The only case of failure from it in my experience was one* in which the body of the uterus had already begun to slough when the repositor was applied, and the pressure increased and extended the gangrene, so that the use of the instrument had to be left off. I have heard of a case, on credible authority, in which the pressure of Aveling's repositor led to perforation of the peritoneum and so to death. So that there are objections to Aveling's repositor.

At the time of the introduction of Aveling's repositor antiseptics were a matter of choice; some believed in them and some did not. Now everybody uses them, and the mere opening of the peritoneum has become attended with little danger. Dr. Berry Hart has reduced a case by opening Douglas's pouch, stretching open digitally the neck of the uterus and then reducing the body of the uterus by taxis. I have published a case † in which I removed a fibroid which had inverted the uterus. The patient was very thin, so that the uterus could be manipulated through the abdominal wall. I dilated the neck of the uterus through the abdominal wall and replaced the uterus by taxis.

In the present case we decided to try first Aveling's repositor, and accordingly this instrument was applied and kept on for thirty-six hours. At the end of that time there was copious hæmorrhage from the uterus. I have not yet met with great hæmorrhage from the uterus produced by Aveling's repositor, therefore that was unusual. The reason that the repositor produced hæmorrhage

was that it, by that time, had got into the body of the uterus. It is the body of the uterus, not the polypus that bleeds. The polypus acts as an irritant, attracts the blood to the body of the uterus, and causes excessive bleeding from the endometrium. The repositor came to press upon the vascular endometrium only when the inversion had been reduced, and that is why the bleeding did not occur at first. Dr. Wilson reported that the repositor had pushed the body of the uterus up within the cervix, but had not reduced the uterus. When the repositor was applied, what was taken to be the body of the uterus was outside the cervix uteri in the vagina. Now, after thirty-six hours' use of the repositor, the tumour was within the cervix, but was still there. The fact was, as we found afterwards, that the repositor had reduced the uterus, and what was felt was the fibroid polypus, for which Dr. Waldron had sent the patient up. Perhaps if we had been able to make a good bimanual examination we should have found that the body of the uterus had been replaced, but the patient, as I have mentioned, was very fat. She was anæsthetised, and I made an incision in Douglas's pouch, intending to stretch the neck of the uterus, but on examining with a view of doing that I found that the body of the uterus had been reduced.

It was clear that we had to do with a tumour. It was twisted off with a pair of strong forceps. When I got it away Dr. Wilson examined and reported that there was still a great mass within the uterus. I need not describe all the manipulations, but Dr. Wilson was right: the mass was the inverted uterus. The traction on the body of the uterus in getting away the tumour had inverted it again. It was replaced this time by plugging the cavity with iodoform gauze; pushing in the gauze pushed up the body of the uterus. The patient got well and has remained well ever since.

The case is interesting because it was so difficult to make a correct diagnosis between inversion of the uterus and polypus, owing to the fact that both these conditions were present, and that the patient was very fat.

The next case is also one which gave rise to difficulty on account of its unusual nature. Dermoids in the pelvis are common, and are usually

* See 'Trans. Obst. Soc. Lond.,' vol. xxx, p. 226.

† 'Brit. Med. Journ.,' 1899, vol. i, p. 467.

in the ovary. Dermoids also grow in the cellular tissue; but if you look in books or journals, the number of dermoids of the ovary you will find described is very large indeed as compared with the number of dermoids of cellular tissue. You might, from that, conclude that dermoids of the cellular tissue were clinical curiosities, so few that when described their nature might reasonably be held in doubt. There is a valuable work, which the German editor* playfully calls a 'Handbook of Gynæcology,' which is in five thick volumes, and in that there is an article by Professor von Rosthorn, who has hunted carefully over the records of dermoids, and has only found published sixteen cases of dermoids of the cellular tissue. These dermoids of the cellular tissue are generally discovered by their suppurating. They are inflamed by labour or violence, or perhaps by bacteria wandering into them from the bowel. When inflamed, they increase rapidly in size, cause symptoms, and suppurate. Then they are opened or burst, the case is put down as pelvic abscess, and no one thinks of its being an inflamed dermoid until the discharge of hair shows its nature. And when you are sure that an abscess is a suppurated dermoid you cannot say where the dermoid was until the patient has come to the post-mortem table and been carefully dissected. Most people think these dermoids are of the ovary. I read a paper on this subject fifteen years ago,† and I carefully headed the paper "On Pelvic Dermoid Cysts," knowing the difficulty of stating the precise situation of the cyst. One speaker in the discussion criticised me for speaking of ovarian dermoid cysts, because he was doubtful about the cysts I referred to being ovarian. He was right, except that I had not called the cysts ovarian. Dermoids may be found in any part of the cellular tissue, between the uterus and the bladder, or in the cellular tissue at the side of the pelvis, or behind the rectum. I will not discuss their origin. I know not that it makes any difference, except that their position determines where, when suppurated, they burst. But reports of carefully dissected dermoids of the cellular tissue are so rare that most people assume that dermoids in the pelvis are ovarian,

* J. Veit.

† 'Trans. Obst. Soc. Lond.,' vol. xxvii, 1885, p. 254.

unless they are sure that the ovaries are healthy. As far as my experience goes, I perhaps ought not to say all, but nearly all the cases of retro-uterine abscess which I have seen have been suppurated cysts, and I think dermoid cysts of the connective tissue.

There is another point about these cysts, that when they occur in the ovary they are often associated with other cystic growths of the ovary. This bears on treatment thus: if you have a dermoid of the ovary, supposing that you could remove that cyst or make it close you probably would not have cured the patient, because there are likely to be other cysts growing in that ovary. In the discussion to which I have referred, a speaker objected to the treatment that I advised on the ground that there were nearly always other cysts present, and that therefore the only proper course was to remove the whole of them. Those dermoids which grow in the pelvic cellular tissue are not commonly accompanied by other cysts, but are generally single. A dermoid in the cellular tissue may burst into the bladder, and there are sufficient cases on record to show the accidents to which it may give rise. Hair, bones, and teeth may be discharged into the bladder, form a nucleus for calculi, or set up cystitis. Such a cyst may be spontaneously cured. It may burst into the bladder and then become inverted, so that it projects as a tumour into the bladder, and it may be spontaneously separated and expelled, and so be cured. This has happened, but it is very rare. In other cases that process has been brought about by the surgeon, who has found a hole leading into the cyst, put his finger into that cyst, seized it with forceps, dragged it out and removed it, and the patient has been cured. In other cases the dermoid has discharged into the bladder and become inverted, and then the surgeon has felt the tumour formed by the inverted dermoid and removed it, and in that way the patient has been cured. Thus the curative process may be effected naturally; may be begun, may be completed, or it may be both begun and completed by the surgeon. The bursting of a dermoid cyst into the bladder is recognised by the presence of fat in the urine, fluid at the temperature of the body, but when the urine has been allowed to stand, forming a pellicle of solid fat on the top of it.

If cystitis is set up by the bursting of such a cyst and keeps up, it is likely that stone is or will be formed.

The treatment of a dermoid which has burst into the bladder is to make a free opening through the vagina into the bladder, explore it, remove all foreign bodies in it, and remove the cyst too if you can.

There are cases in which a dermoid has burst into the rectum, has become inverted, and has been expelled, or has been removed by the surgeon. There are also cases, of which I have seen several, in which a dermoid has suppurated behind the uterus and burst into the vagina. When the pus is discharged these cavities contract and become very small. It is commonly believed that if a dermoid cyst opens and discharges, so long as any part of the cyst wall is left it will go on discharging. This should be taken with some qualification. It means that if a dermoid cyst bursts and the bottom of the cyst is fixed, it will go on discharging. But if the cyst is not fixed, can collapse and pack itself into a small space, it will empty its pus and practically cease discharging. Even if it continues to discharge, a small discharging sinus in the vagina is not so inconvenient as one on an external part. This patient was admitted September 30th. The patient dated her illness from six weeks before her admission. A doctor had told her that she was three months pregnant. The patient did not know whether she was or was not pregnant. On August 7th she received a blow on the lower part of the abdomen, and was unconscious for half an hour. She had an ovarian tumour rising out of the pelvis and reaching to the level of the umbilicus. It was elastic, irregular in outline, fluctuating, dull in front, resonant in the flanks. There was a hard swelling in the posterior vaginal fornix.

On October 3rd the tumour was removed in the ordinary way. The tumour was found to occupy the right broad ligament. It was lifted up from the pelvis, the broad ligament stripped off it, and the pedicle ligatured. The main part of it was a dermoid cyst. When removing an ovarian tumour it is a rule always to examine the other ovary. The clinical clerk who watched the operation and wrote the notes, noticed that I

examined the other ovary. The patient went on well until October 11th, eight days after the operation. Her temperature then was 102°, and she complained of pain. Dr. Wilson examined her and found a firm fixed swelling in the left iliac region, running up to within two fingers' breadth of the umbilicus. There was no swelling of this size at the time of the operation. The uterus was felt to be distinct from it. These symptoms continued, and on October 17th the patient was anæsthetised and the posterior vaginal fornix opened, with the view of ascertaining what the lump was. Some pus was let out, and within the cavity a firm solid mass could be felt. This mass appeared to lie free, and various conjectures were made as to its nature. One surmised that it was a clot, another asked if it could be a sponge left in the abdomen (but the sponges had been counted and none were missing), and another suggested that some viscus might have become displaced, for there have been cases in which the spleen and kidney have been displaced into the pelvis. But there was nothing which gave any definite indication of what the condition was. It was thought that letting out the pus might be enough. After the letting out of the pus the patient, for a time, felt better, but after a few days she began to get worse, and on October 22nd the incision was reopened and more pus let out. Still the lump could be felt unaltered in character. I pulled it down to see what it was, and it turned out to be a dermoid cyst, no doubt of the pelvic connective tissue. It was composed of fibro-cellular tissue covered with skin, and attached to the skin there were hair and bone. I removed by torsion as much of it as I could reach, and after this the patient was much better. The temperature came down, the discharge became scanty and less offensive. On November 28th the patient was so far well that it was a question whether she should go home. I thought it well to examine her before she did. The old incision was opened and the cavity explored, and then I felt some more solid substance and got away three or four teeth with some bone and a considerable amount of fibro-cellular tissue. After this the patient improved steadily, and at the time she left the hospital, while I could not say there was no dermoid tissue there, I knew there was not enough to be

obvious as a tumour, and I do not suppose the patient will be caused further trouble by it.

Of course the presence of such a tumour ought to have been ascertained at the time of the operation. The other ovary was examined and found to be healthy, and the possibility of there being another tumour which was not ovarian did not occur to me at the time. A similar thing has happened before, for in the valuable atlas of female pelvic anatomy, published by Professor Clarence Webster, there is a picture of a frozen section of a patient who died a few days after operation by an eminent gynaecologist for the removal of a dermoid cyst. The section shows another dermoid cyst in the pelvis, under the peritoneum, in the connective tissue which had not been noticed at the operation and had therefore not been removed. Such cases as this are very rare. It is unusual to have a combination of dermoid cyst in the ovary and one in the cellular tissue, and still more unusual for the connective-tissue cyst to begin to suppurate ten days after the removal of the ovarian cyst. As an example of a rare condition and coincidence the case is of interest.

A CASE OF CHYLO-THORAX.

BY

H. W. SYERS, M.A., M.D.Cantab.

THE patient, a male æt. 52, had been ill for six weeks. The illness commenced with stabbing pain extending from the left scapula through the chest to the front of the same. The pain was always more severe on drawing a deep breath. His breath had been getting gradually shorter from the commencement of the illness. He had lost flesh during the six weeks preceding admission, and there had been profuse sweating at night during the same period. His previous health had always been good, in fact there was no history of antecedent illness. One brother died of phthisis. He was a healthy-looking man, making no complaint of pain, and none of dyspnoea so long as he remained in bed and lay on the left side. Examination of the chest showed that the expansion was freer on the right than on the left side. On percussion the left chest was dull, back and front; in the former situation

as high as the spine of the scapula, in the latter to the level of the third interspace. Over the left base vocal fremitus was absent, and on auscultation it was found that the breath-sounds were all but inaudible. In fact all the physical signs proper to a large pleural effusion were present. The heart was considerably displaced to the right. Examination of the abdomen revealed no abnormality, and that of the urine gave negative results. A few days later aspiration of the left pleural cavity was performed, and twelve ounces of fluid were removed. This fluid was opaque and of a yellowish colour, looking as if composed of semi-purulent matter. The physical signs remained as before, being unaffected by the removal of the fluid. Ten days later an exploring syringe was used, and a tube-full of thin, opaque, dirty-coloured fluid was withdrawn from the left pleural cavity. Microscopical examination of this fluid proved the entire absence of pus cells, but a very large number of fat globules were found, varying in size from that of a pin-point to a quarter of an inch in diameter (as seen under a one fourth objective). In addition there was a quantity of granular detritus, and a number of epithelial cells. With osmic acid this fluid stained deeply black. A week later the syringe was again employed, and fluid drawn off resembling milk or chyle, and which gave precisely the same appearance under the microscope. At this time a prominence was noticed outside the ribs at the left base, which was neither red nor tender, but which was perfectly visible to the naked eye. The patient declined any surgical interference, and he left the hospital just four weeks after admission. With the exception just referred to (the presence of the prominence over the left ribs) the physical signs were practically the same as those observed when he came into the hospital.

This case when first seen was thought to be one of ordinary pleural effusion, possibly purulent, and the patient was sent into hospital with the view of making an exploratory puncture and then proceeding to surgical measures if necessary. At the first paracentesis the appearance of the fluid supported the presence of pus, and it was considered that the physical signs were probably due to empyema. But when the physical signs in no way improved, and the patient

was seen to be unrelieved, it was necessary to again explore the pleural cavity. Microscopical examination of the fluid now withdrawn at once proved that no empyema was present, but that the effusion was of a chylous nature. Further treatment being objected to by the patient, he left the hospital. About one year later he came again as an out-patient, saying that during the period which had elapsed since he left the hospital he had been fairly well, at work, and had nothing particular to complain of until a few weeks before his return to the out-patient department. The old troubles had returned during this period, and the shortness of breath had become so urgent that he was induced again to seek advice. Examination showed almost precisely the same condition of things as when he first presented himself. The prominence over the lower left chest had disappeared, otherwise there was practically no change. The physical signs were those of considerable pleural effusion. He was disinclined to enter the hospital, and was treated as an out-patient. When last seen he said the shortness of breath was less severe, and that he felt better in all respects. There can, I think, be little doubt that the case is one of that rare condition, chylo-thorax. The diagnosis of empyema is excluded not only by the result of the examination of the effusion withdrawn by the exploratory aspiration, but also by the subsequent history of the case. It seems scarcely credible that, had the fluid in the pleural cavity been pus, matters would have remained in a quiescent condition for two years, without pointing externally or through the lung. What may have been the nature of the communication between the thoracic duct and the left pleural cavity is quite uncertain. There was absolutely no evidence on which to found an opinion. It may be that ulceration had caused a small perforation of the duct, but it is impossible to suggest the nature of such ulceration. That the passage of the contents of the duct into the pleural cavity was not continuous or in large quantity seems certain, for the physical signs did not become more marked, neither did the nutrition of the patient suffer during two years. It can only be supposed that the communication, whatever its nature, between the thoracic duct and the pleural cavity, must have been small, and that subsequently it

must have been sealed up. As far as I know very few cases of this description have been recorded, and we know from practical experience that the condition is very seldom met with.

THE RESPIRATORY AFTER-EFFECTS FOLLOWING THE INHALATION OF ETHER.

A discussion at a Meeting of the Society of
Anæsthetists.

The President, Dr. J. FREDK. W. SILK, in the
Chair.

(*Concluded from p. 271.*)

DR. FLUX said there were two causes which might possibly at times give rise to respiratory trouble during and after ether administration which had not been alluded to that evening, and they had been rather intimately forced upon his mind, though he had not himself had special difficulty in that respect. The first cause which one rarely saw mentioned—in fact, he did not remember having seen it mentioned—was one which he was sure happened from time to time, namely, the inhalation of liquid ether. Where the apparatus used was either faultily constructed or was old or damaged this inhalation of liquid ether would occur. It could occur with certain forms of Clover's inhaler, and it could also occur when an old sponge was used in an Ormsby's inhaler, especially if great care was not used in starting the administration. Not only might liquid ether run into the patient's mouth—he had known it occur and seen it expelled—but another cause of trouble was the violence which was sometimes employed when ether was administered. Dr. Buxton had said that it was in hospitals that undesirable after-effects were most frequently noticed. He, Dr. Flux, thought that might possibly be brought about from the fact that in the hospitals more cases were anæsthetised by those who were not experts than in private practice, where experts were specially provided. With the uninitiated and students there was often a very violent administration of ether, and the expirations and inspirations were sometimes ex-

treme, so much so that the bag, whether of a Clover or an Ormsby, if not a large one, would be distended to such a degree in expiration that there must be considerable back-pressure on the lungs themselves, and in the case of a large strong man, as in the case which had been mentioned in St. Thomas's Hospital, the patient in inspiration would tend to collapse his lungs and also to draw liquid ether from the apparatus by suction. When attempts were made to get patients under the influence of the anæsthetic hurriedly he thought that those points were apt to be lost sight of.

Mr. TYRRELL said he had been asking the surgeons whom he had been working with lately whether they could remember any cases of respiratory troubles following ether, and most of them answered in the negative, or stated that if there were any they occurred so long ago that they could not remember them. He, Mr. Tyrrell, strongly believed that such cases did occur, and he asked Mr. Corner to come and give instances of cases. He, Mr. Tyrrell, knew such cases occurred less seldom in hospitals than in private practice, because in the latter case experienced anæsthetists were retained. He believed that rushing the ether to begin with had something to do with it, and so had the giving of large quantities of ether. In Mr. Corner's case, Mr. Corner told him that the patient required an enormous quantity of ether. Hurrying over the administration was apt to cause the secretion of a lot of mucus, which got sucked into the bronchial tubes. To avoid that mucus getting sucked into the bronchial tubes he, Mr. Tyrrell, thought the position mentioned by Dr. Hewitt, a position which would drain the mouth, was of very great service, and he, Mr. Tyrrell, was in the habit of putting into the corner of a patient's mouth an absorbent towel, and getting the head on one side. Occasionally so much mucus got into the trachea that he had not infrequently asked the surgeon to allow the patient to come round sufficiently to cough and retch. Sometimes after twenty minutes' anæsthesia there was so much rattling of the mucus that if the patient were allowed to come round sufficiently to cough, the end of a long operation would often be reached without any trouble whatever.

He would like to tell them a story, which was

of importance as showing that they should follow up their cases in which it was supposed after-effects were due to the anæsthetic. A case occurred to him not long ago in which an abdominal section was about to be performed, and it was thought by the surgeon that he would have to do an anastomosis of the bowel. Chloroform was specially asked for by the patient, and he, Mr. Tyrrell, commenced with that only. The whole of the bowel was pulled out inch by inch, the flexures were all gone over carefully, and the area under the liver was felt, and at that time the patient lost considerable colour. He therefore turned on a very little ether from the double-bottle Junker apparatus which he always carried. The patient improved, and went on all right to the end of the operation. He did not use more than two or three drachms of ether. He left the patient one and a half hours after the first incision, when the stitches had just been inserted, and the patient appeared to be in fairly good condition. He heard afterwards that she had vomited for two days. The next report he heard was that he had been tried by the surgeon and practitioner and convicted of ether pneumonia. The patient had been tapped and fluid let out of the pleura. The next report he heard was that he had been retried by the same tribunal, a large subdiaphragmatic abscess had been opened, and he, Mr. Tyrrell, had been exonerated. When he heard the patient had been tapped he did not believe it was due to the ether or to the chloroform. He believed it was due to a germ, which might possibly be introduced from the face-piece. The fact that the abscess was subdiaphragmatic proved that it was neither the one nor the other, but a flaw in the asepticism in the surgical procedure.

Mr. CROUCH said he wished to report a case of death from ether, which he thought might be of interest to the Society. On July 23rd, one of the hottest days last summer, at ten in the evening, he was asked by a physician to give an anæsthetic to a patient who was halfway through a miscarriage. She was a young lady thirty-two years of age, who a week before had had the commencing trouble, since which time she had been discharging a lot of foetid pus. The general practitioner in the case treated her with large doses of quinine. When he, Mr. Crouch, saw

her he was informed she had had a rigor with a temperature of 104° . He found the pulse extremely good and the temperature normal, with no physical signs at the front of her chest. He gave her gas and ether. She went off fairly quietly and quickly and required only a small quantity of ether to keep her under. At 10.25 the operation was commenced, and at 11 o'clock it was over, an extremely offensive ovary having been removed. She was put back to bed and immediately started a sobbing respiration, which lasted twenty minutes and then stopped. The pulse and colour of the patient had been good the whole time, and the surgeon was extremely pleased with the condition. He, Mr. Crouch, then left, and the next morning he heard she was dead. It appeared that twenty minutes after he left, the patient's noisy and moist breathing attracted the attention of the surgeon. He stayed half an hour giving injections of strychnine, but she died two hours after the operation was completed. The surgeon told him, Mr. Crouch, about it next morning, and asked him his opinion about the case. He said he thought death had been due to acute œdema following ether, and that he, Mr. Crouch, was responsible for it. However, the surgeon in the case was, two days later, called to empty another uterus. The patient was put under chloroform and remained under it half an hour. Exactly the same sequelæ of events occurred; the patient died two hours afterwards, although she had had chloroform. He, Mr. Crouch, went to give an anæsthetic in the country where there was a doctor of large experience in midwifery, to whom he related those cases just mentioned. He replied, "Well, of course it may be very curious to you, but these big men in London do not know anything. I have seen lots of miscarriages occur and death come on without any anæsthetic having been given. It is not the anæsthetic at all which produces the death; it is the pathological condition following the shock of emptying the uterus." How far that practitioner was right he did not know, but he was able to find in his case books three instances of women who had miscarried and in two to five hours had died of acute œdema of the lungs. He reported those cases, because, at the time, death was thought to be due to the ether. It might have been, but in

view of what he had said it was possible that the death might have been due to other causes.

Dr. LAW said he had only had one case of the kind under discussion, namely, at St. Thomas's Hospital, about eighteen months or two years ago. The operation was gastrostomy. There was considerable difficulty in getting the stomach forward. The patient took the anæsthetic exceedingly well, and when he left the table, he, Dr. Law, noticed that his condition was much better than usual after an operation of that kind. About three hours afterwards the patient began to get blue, and the resident physician was sent for to examine him. He found both bases quite dull. He got worse, and died during the night. A post-mortem examination was made, and the major part of both lobes was absolutely airless. Whether the ether caused œdema of the lungs he did not know. Certainly at the time of the operation there was no difficulty and there was no mucus, and when he left the table the patient had no vomiting at all. Possibly the manipulation of the stomach had a reflex effect on the vagus and thence upon the pulmonary system. It did not seem to be due to ether itself.

Dr. DUDLEY BUXTON said he would like to thank Mr. Corner for the very interesting communication, and to disabuse Mr. Corner's mind of an idea, which he had possibly acquired from his, Dr. Dudley Buxton's, lack of clear expression. What he intended to convey was, not that all cases of respiratory trouble were due to carelessness and exposure in hospital, but that in many cases exposure in or out of hospital was responsible for a patient developing respiratory trouble. He thought Mr. Corner would admit that the fact of two cases with which he had been brought into contact giving no evidence of exposure did not invalidate the position which he, Dr. Dudley Buxton, took up. He thought Mr. Corner had supported that position by showing that in the case of two patients the ether was distinctly given not wisely and perhaps not too well. Possibly, as was commonly the case, those who gave chloroform mostly were more dextrous with the use of that drug than with the ether; and hence the moral of those two cases.

The PRESIDENT said that at that late period of the evening the members would not expect him to say much with respect to the very interesting

discussion which had been carried on. His first duty was to thank all who had come forward to contribute to the consideration of the question before them. At the same time he regretted they had not been more enlightened from the surgical point of view. The members of the Society naturally laid themselves open to the accusation of special pleading, unintentional in some cases, perhaps intentional in others, for they were sometimes told that it was to their interests, or they thought it was, to minimise the effects of ether upon the pulmonary system. It seemed to him somewhat of a pity that the surgeons who held the opposite opinion did not come to the meeting in force to bring their view of the case before the Society. As his, Dr. Silk's, contribution to the discussion, he would like to pass round a chart which was compiled from the King's College Hospital records. He did not wish to dilate upon it, because it was exactly similar to the cases that had been described by other speakers. It represented the solitary instance of pulmonary trouble that had occurred in at least 900 operations which had been performed in a given year at King's College, and this was the only case of bronchial trouble which was of sufficient severity to impress itself upon the temperature chart and clinical records. He thought it might yield a good estimate of the proportion of cases in which respiratory after-trouble occurred. There was no feature in the case itself to which special attention need be drawn.

There were two points which seemed to him to have been established by the discussion, though he was not quite certain as to their exact bearings. The first was the age of the patients in whom pulmonary troubles occurred. Practically all such patients were comparatively young. Mrs. Stanley Boyd was good enough to come forward last month and give a series of cases of the greatest possible interest, whose ages were 25, 35, 36, 34, and one of 65. The average age of the cases appeared to be about 32 to 35 years. It did not appear to be in the very old that the respiratory complications after anæsthetics seemed to arise. Another point which seemed to have come out in the discussion in a very marked way, was the preponderance of abdominal cases. A very large number of the cases which had been quoted had been those in which the abdomen

had been interfered with, and the explanation was probably very largely the one suggested by Dr. Dudley Buxton: that if the man's respiratory powers were interfered with by opening the abdomen, that must have a great effect on his capacity for getting rid of any mucus which might be in excess.

He thought a matter to which attention should be drawn was the relative proportion of cases in which pulmonary trouble occurred in times gone by—in the pre-antiseptic and pre-anæsthetic days. If they took the trouble to consult one of the surgical text-books of the pre-antiseptic days he thought they would find that a very large proportion of the deaths after operation were attributed to pulmonary œdema and other lung troubles. They might of course have been of a septic character, and it did not seem to him to be altogether fair that because antiseptics had been introduced the trouble should now always be assigned to the anæsthetic. There was such a thing as failure of antiseptics, and to that failure he thought the difficulty was sometimes due.

That brought him to speak of the question of the relative proportion of cases. He concurred with Dr. Dudley Buxton in his remarks on this point. He had himself found it almost impossible to arrive at anything like a conclusion as to the proportion of cases of bad after-effects. If it were one in 300, as stated by some in America, then he would be inclined to say it was due usually to the method obtaining in America of drowning the patient in the anæsthetic in order to get him under. He believed the proportion was not anywhere near that figure.

The only other remark he wished to make was to emphasise in the most emphatic way that he could Dr. Dudley Buxton's view as to the misleading character of the term "ether pneumonia." He thought it was a most misleading term and quite wrong, because it implied, first of all that there was something *sui generis* in the pneumonia or other pulmonary trouble, which was seen after the administration of ether. He agreed with Dr. Dudley Buxton that there was nothing of the sort, but that such troubles as ensued were the troubles which occurred in the ordinary course. What was much more important was that such a term implied that the pneumonia was of the croupous variety. He felt sure that croupous pneumonia did not occur after ether with any greater frequency than in ordinary every-day life. People who spoke of ether pneumonia seemed almost to forget that pneumonia was an extremely common disease, and that a very large number of deaths were due to it. He was almost inclined to think, with Mr. Carter Braine, that the inhalation of ether was, if anything, protective against pneumonia.

THE POTENTIAL ENERGY OF THE BLOOD-CORPUSCLES.

BY

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In an article on the determination of sex I made the suggestion that, in a small animal, diagrammatically represented thus—



the ovum possessed a certain amount of potential energy, or energy for reaction, sufficient to cause its division into two cells, which, by interaction, produced two more, and similarly these four gave rise to two others, when the potential energy, being diminished, resulted in the production of only one cell, and that of increased energy—the ovum.

Each cell, thus participating, possesses a certain amount of potential energy, and any interaction between these cells represents a certain interchange of potential energy.

If the animal be small, as in the diagram, the interaction, or the interchange of potential energy, is practically direct, since all the cells are almost in contact one with the other.

If, however, the animal be of slightly larger size, such direct interaction is impossible, and there can only be indirect interaction.

This indirect interaction between two cells, at opposite ends of an animal of this kind, requires, therefore, some addition to the organism, as, for example, a fluid which can move to and fro amongst all the cells, carrying in it the products of interaction, which fluid of necessity represents a certain amount of potential energy.

For small amounts of potential energy a comparatively simple fluid is all that is required, but as we ascend the animal series we find the fluid, having increased duties to perform, and being charged with a greater amount of potential energy, necessarily becomes more elaborate and complicated.

Ascending still further, we find the complicated fluid aided in its function by the presence of simple cells, the colourless corpuscles.

Higher still, some of the colourless corpuscles get added to them colouring matter in the form of what is called "hæmoglobin."

As the duties of the corpuscles become still more onerous, the amount of colouring matter becomes still greater, and eventually we find that what in the lower animals is contained in one corpuscle, is, in the higher animals, represented by a coloured and a colourless corpuscle; in other words, the coloured and colourless corpuscles in the blood of mammals are represented by the coloured corpuscles of a larger size in the lower animals, the elaborate leucocytes of mammals being represented by simple forms of leucocytes.

A natural conclusion from this fact is, that the coloured corpuscles are the satellites of the colourless corpuscles.

One finds the colourless corpuscles unaccompanied by the coloured in many parts, but never the coloured without the colourless.

As the quantity and quality of the rations with an army are, to some extent, a criterion of the possibilities of that army, so the quantity and quality of the coloured corpuscles may be taken as a criterion of the state of the colourless corpuscles, or, in other words, of the state of the bodily health.

I venture to suggest that the process, which is to be found in bone marrow, namely the large pale cells developing colouring matter in one part, and then separating it in the form of coloured corpuscles, represents to some extent the evolution of the coloured and colourless corpuscles.

If the original simple fluid was destined for the interaction of distant cells, and for an interchange of potential energy, how much more so must the more highly developed blood of mammals be destined for such an interchange of potential energy, and therefore it must itself, and especially the corpuscles, be charged with a certain amount of potential energy, and on the state of this corpuscular potential energy, and its relation to the potential energy of the various cells of the body, depends whether that body possesses good or bad health.

Action has its equivalent in reaction, and so with whatever energy the corpuscles act on the body, with an equal amount of energy do the tissue cells of the body react on the corpuscles.

As long as this equalisation of energy goes on uninterruptedly, so long do we find the body in a state of sound bodily health.

Anything which tends to modify the meta-

bolism of the corpuscles, modifies the interaction of the energies; and if the modification is sufficiently great, then we find the body in a state of disease.

A rapid modification of the metabolism of the corpuscles produces a rapid change in the metabolic products, which products, reacting on the cells of the body, cause them also to produce products which, in turn, react prejudicially on the corpuscles.

Such a rapid change is contemporaneous, is, in fact, synonymous, with an acute disease.

A slow change of a similar nature means chronic disease.

An acute form of such a change of metabolism is to be seen in coagulation of the blood. In this process we find the physical conditions altered, *e. g.* contact with air, and absence of circulatory movement, whilst there is also a loss of that exchange between the corpuscles of the cells of the body.

The result of such a change in the conditions is that the metabolism is altered, and, of course, the metabolic products are altered, so that now we find a substance produced which affects the fibrinogen, and converts it into fibrin. Possibly, circumstances have caused the production of a coagulating, instead of an usually non-coagulating, ferment.

Such an effect as the production of this altered metabolism, and metabolic product, is delayed by the addition of certain salts to the blood, or by supplying it with the living cells of a blood-vessel, which can keep up, to some extent, a sufficient amount of interaction between the cells.

Feed the leucocytes with a proteid from the thymus, and we find metabolism so altered as to transform very rapidly the pro-thrombin into the thrombin.

It is a well-known fact that the blood of one kind of animal, injected into a bled animal of another kind, will not sustain life, the reason for which, I venture to suggest, is not so much the fact that it contains products inimical to the cells of the new animal, but that there is no possibility of equalising the action and reaction of the corpuscles with the cells of the body, the products of the corpuscles being detrimental to the metabolism of the body cells, whose products on the other hand are detrimental to the metabolism of

the corpuscles. Were the life history of the corpuscles sufficiently long to allow of an adaptation to their new surroundings, life might be sustained.

Such a modification of the corpuscular metabolism, however, proceeds every day throughout the whole animal economy.

The metabolism of the corpuscles, which is of such a nature as to react satisfactorily with the cells of the infant body, is not the same as that which proceeds in the corpuscles of the adult, for the metabolic products of the adult body-cells would soon react disastrously on the infant corpuscles; from which one might infer that, throughout life, a steady constant change must be taking place in the form of the metabolism of the corpuscles, or, in other words, there is an evolution of the corpuscles.

The corpuscles which would lead, directly or indirectly, to the growth of bone, are not those which would be required when the bones have ceased growing; a degeneration of the adult corpuscle results in a reversion to an earlier type, which deposits calcareous matter, as seen in the calcification of sarcomata.

Such an evolution of the corpuscle might be said to run contemporaneously with the evolution of the skeleton of the vertebrata.

An instance of an experimental alteration in the interaction of the corpuscles and the cells of the body is supplied in the case of lead poisoning, where, as a result of introducing a foreign salt, certain tissues react in such a way as to deposit urates in certain joints, at the same time as they react on the life of the corpuscles, so affecting them as to give rise to what we call *cachexia*.

This introduction of fresh energy, in the form of lead, results in an interference with the normal potential energy of the corpuscles (which represent health), and we find a disease produced, which we call *gout*.

The natural deduction is, therefore, that, for the production of gout, an external energy must be introduced to interfere with the normal dissipation of the energy of the corpuscles, or, in other words, their metabolism. Such energy is supplied by the different articles of high living, or beer-drinking.

Hereditary gout may be mentioned as an exception, but here we find another source of potential energy in what is usually called *predisposition*.

Heredity may be defined as the transmission of a special breed of corpuscles with their special predispositions.

Hence there is in this form of gout sufficient innate energy, without the necessity of very much external aid, for an erratic reaction to take place between the corpuscles and certain cells of the body, to wit, the cells of the joints.

Let us consider for a moment the case of a local phlebitis, produced by a small change in the local conditions as, *e. g.*, by a slight injury to the wall of the blood-vessel. The result is the formation of a clot round the injured part. We have no reason to believe that the formation of such a clot takes place differently from that of a clot outside the body. We have here a change sufficient to damage, or interfere with, the workings of one side of the equation, so that a variation must occur (not necessarily directly or inversely, to complete the metaphor) in the metabolism of the colourless corpuscles to such an extent as to result in the formation of the clot; this clot is only formed within such a radius as the change of conditions can effect, but within that radius the changed metabolism produces the ferment necessary for coagulation: but is that all?

In studying the subject of coagulation, we have no instrument as sensitive even as a galvanometer, to record changes other than chemical, but in the human body we have such a sensitive recorder, with the result that we find, not only does this ferment or cell-product cause coagulation, but also a rise of temperature.

This rise of temperature must be for some object, which, no doubt, is to stimulate the circulatory system in such a way as to help the locally disturbed corpuscles, so as to enable them to recover their former and normal metabolism.

Such, I take it, is the response of the circulatory system to the disturbance caused by a simple injury to the walls of the blood-vessel—the corpuscles produce a ferment, which forms a clot, and causes a rise of temperature. Can we call this ferment the simplest example of an antitoxin?

Complicate the circumstances, by substituting this slight injury, by organised germs of a disease, and we find the result of the experiment similar in nature, but more intricate.

Take, for instance, a case of diphtheria. Here we find the introduction of the poison leads to the

concentration of the corpuscles (as described by Metschnikoff), with the result that they have their life threatened, or, in other words, their metabolism is disturbed, leading to a production of a ferment, which causes a sort of clot—the diphtheritic membrane—and also a rise of temperature—but is that all? Is the simple reaction due to the injury merely reproduced? Certainly not, the difference being shown by the more complicated nature of the “clot,” where not only does the ferment act on the proteids of the blood, but also on the cells of the tonsils; it produces not only a rise of temperature, but also a damaging effect on the kidneys.

In the simple example of phlebitis discussed above, what would be the result of a failure of the stimulated circulation to restore metabolism to its normal state in the leucocytes?

The wave of stimulation is succeeded by a wave of depression, and the result would be a failure of the circulation, not only as to the special function it has been called upon to perform, but also as regards its normal function of maintaining the health of all the cells of the body; in other words, a collapse or even death.

In diphtheria the failure of the circulation to re-establish the disturbed metabolism of the corpuscles affected by the disease poison, would result in a depression, a collapse complicated by the additional damage done to the cells of the kidney.

Possibly the ferment produced in diphtheria is of such a nature as to be a poor or insufficient stimulant of the circulation, but a powerful disturber of the kidney cells. We want, therefore, for treatment, the counterpart, an antitoxin which will stimulate the circulation powerfully, but will hardly damage the kidney, and that is what we find in the antitoxin derived from the blood of the horse.

The corpuscles of the horse metabolise differently from those of the human body, and of necessity, therefore, react to stimuli, to physical conditions (as seen in delayed clotting) and to organised poisons, in a different way also, producing in this last case a different form of antitoxin, or ferment.

In scarlet fever we find a condition of the throat somewhat similar to that in diphtheria. The disease poison affects the metabolism of the corpuscles, in such a way as to produce at least

a ferment which seems to make an attempt at producing a clot, or a condition of that nature in the form of the yellowish points, or patches in the throat—certainly a considerable modification of the clotting process, but a modification notwithstanding. The ferment produces also a rise of temperature, an alteration in the kidney cells, and a modification of the reaction between the corpuscles and the cells of the skin, causing a rash.

(It may be mentioned here that the antitoxin used for diphtheria sometimes produces a rash.)

The reaction, also, between the cells of the joints and the corpuscles is modified as exemplified by the rheumatic affection occasionally seen.

The variation in the amount of "ferment" produced will account possibly for the varieties of type of scarlet fever—such a quantitative variation being due either to the quantity of the disease poison, or to its strength and activity.

In comparison with diphtheria, it would appear as if the metabolism of the corpuscles were affected to a greater extent in scarlet fever, both quantitatively and qualitatively, which may be a reason for the *greater immunity* caused by an attack of the latter disease than that caused by an attack of the former; for if their metabolism is so thoroughly affected throughout the system, a display of energy in that direction is not so likely to occur again.

The varieties of rashes in the several acute diseases may be, therefore, accounted for by the different "ferments" produced, and their different effects on the action and reaction between the corpuscles, and tissue cells, in different parts of the body.

The disturbance of equilibrium does not necessarily follow in all parts of the body, for the mutual interaction of corpuscles and cells, in a certain part, may be sufficiently strong and active to proceed without any interference, at least for a time; or, on the other hand, no such interference may occur at all, the whole energy of the "ferment" being used against the corpuscles themselves, as possibly occurs in malaria.

In malaria it seems as if, for a certain time after the introduction of the poison, a marked change in the corpuscular metabolism takes place, possibly producing a "ferment" which, in addition to causing a rise of temperature, does not affect the interaction of the corpuscles, and other cells,

though it may react on the corpuscles themselves, for whether due to their elaborate and changed metabolism, or to the action of the "ferment" on them, the corpuscles become distorted and broken up.

Destruction of blood-corpuscles, as in hæmoglobinuria, appears to be accompanied by a feeling of chilliness or rigor, and, quite possibly, the destruction of white corpuscles in malaria is also attended by a rigor.

To explain the various forms of malaria, one has the right to assume that, as in other diseases, the power of the virus varies.

It would depend on the strength of the virus as to whether it could affect the corpuscular metabolism, and, if so, whether the metabolism is affected quickly or not, the quality of the metabolic product—the "ferment"—varying according to its rate of production.

A certain metabolic rate for each individual, or a sort of personal equation, must also be considered, for the corpuscular metabolism of one person may be more susceptible to, or vary more for, a certain stimulus, than would that of another. So that the time necessary to produce the "ferment," and rupture or disorganisation of the corpuscles in one case, would be shorter, or longer, than the time in another case.

To explain the characteristic enlargement of the spleen in malaria, I would suggest that it is due to the immense destruction of colourless corpuscles, and an elaborate attempt on their part to recover themselves by an immense devouring of coloured corpuscles.

Normally in the spleen are to be seen colourless corpuscles in the act of devouring the coloured.

A congested or inflammatory state of the spleen suggests an attempt by nature to increase such opportunities for feeding the colourless corpuscles, which are so conspicuously damaged in this disease.

One might facetiously compare the marrow of bones to the cookhouse, where are prepared the coloured corpuscles in a form suitable for presentation to the colourless corpuscles, in the dining-room of the spleen.

As the growth of the infant is so closely connected with the action of the corpuscles, so must, I take it, any change or modification of growth be closely connected with the action of the corpuscles, as *e. g.* the changes which occur at *puberty*.

The potential energy or evolution of the corpuscles has produced (without any external aid, as *e. g.* the energy of the virus) a state of affairs where, the corpuscles being altered, their reaction or interaction with certain cells is altered to such an extent that the metabolism of those cells is altered also.

An altered metabolism of the corpuscles results in altered metabolic products, as stated in discussing diphtheria and scarlet fever, the product, or "ferment," producing in the latter case a typical rash.

The altered metabolic products of puberty result at times in an affection of the skin of boys and produce *acne*—a sort of semi-acute result of the dislocation or disturbance of the corpuscular metabolism.

A more chronic, or rather a permanent, result of such a change of corpuscular metabolism, and, consequently, of action and reaction with the cells of the skin, is the growth of hair on the face.

In girls, the potential energy of the corpuscular change of metabolism is sufficient to cause the cells of the mammary gland to react differently, and so to increase in size.

A similar change may be said to take place as regards the uterus.

All these modifications are due to the arrival of the corpuscles at a certain stage of their evolution, or, in other words, due to a modification of their evolutionary energy.

Contemporaneously with the arrival of puberty occurs the function called *menstruation*, which is said to be due to the proliferation of the cells lining the uterus, which eventually break down.

I would venture to suggest that this process is due to a lack of sufficient energy, in the metabolic interaction of the corpuscles and lining cells, to make the process permanent.

If energy is introduced in the form of a spermatozoon, the metabolic activity so produced in the ovum results in a *product*—(is it a "ferment"?)—which so energises the corpuscles, in their reaction with the cells of the endometrium, as to make the proliferation preceding menstruation not only permanent (comparatively), but also causes it to further develop, up to the next stage, that of "failure" or childbirth.

Seeing, therefore, that the metabolic "product" of the ovum, after the entrance of the spermato-

zoon, is the cause of the development of the decidual lining of the uterus, may we not infer that the metabolic "product" of the unfertilised ovum is the cause of the menstrual proliferation, which, accompanied by the usual extra supply of blood-vessels near a growing part, on breaking down, causes bleeding from these young and delicate blood-vessels?

If, then, at puberty we find the potential energy of the corpuscles producing growth of endometrium, uterine muscle, and mammary cells, it is easily understood how the added energy (due to the entrance of the spermatozoon) can result in still further growth of endometrium, uterine muscle, and mammary cells, up to a point where the energy can carry their development no further, and they all break down, and their products (milk, etc.) are got rid of in different ways.

The potential energy of the corpuscles of the same class of animal being practically the same, but different from that of other animals, we can see how the limitations and variations of the energy of the corpuscles, in pregnancy, cause the periods of gestation to vary in different animals, but to be of the same length in animals of the same class.

If the energy introduced by the entrance of the spermatozoon can result in such a tremendous change of metabolic activity and interaction between the corpuscles and certain cells more or less ready to respond, it is easy to understand how the energy introduced with the virus of syphilis or tubercle produces its effects.

In pregnancy we find cells prepared to respond to the stimulated or increased energy; in syphilis we do not (at least not to the same extent).

In pregnancy the proliferating endothelium can only find its syphilitic counterpart in the perpetually proliferating epidermis.

If infection occurs, if a response is made by the cells of the epithelium, we find the proliferation locally exaggerated; in other words, a hard chancre.

Epithelial metabolism, being thus affected, leads to altered metabolic products, which, acting on the corpuscles, seem liable to affect their reaction with the epidermis still further, and so we find a rash produced.

If the energy of the disease be interfered with by any outside energy, such as the introduction of a mercurial salt, the course of events may be altered.

If mercurial treatment has been neglected or has been insufficient, the products of the stimulated corpuscles are strong enough to affect the action of other cells, and so we find the syphilitic gummata.

To interfere with this erratic display of energy, we have to introduce another form of energy in the shape of iodide of potassium.

The rash of the iodide is sufficient proof of its effect on the metabolic interaction of corpuscles and epidermic cells, and energy can be diverted, or interfered with, only by energy.

In tubercle, the energy of the virus is so slight that it cannot affect even the proliferating skin, but must wait (unless it perish in the interval) for a weakened, or damaged spot, this giving rise to the necessary proliferation.

In tubercle the element of added energy in the form of predisposition comes into play, and these two energies acting together make treatment more complicated and difficult.

In a person predisposed to cancer, we find a state of affairs where the energy of the corpuscles has added to it the additional energy (however slight or great) of predisposition.

But if in tubercle the weak energy of the virus must be added to that of the predisposition, so in cancer some additional energy in the form of some product of an erratic metabolism is necessary to cause the necessary interference with the normal interaction between the corpuscles and those cells.

This, I take it, is supplied by the damage done to the cells of that part by a severe blow, or by a constant irritation which produces an alteration in the cell metabolism, as seen in the proliferation attendant on repair, during which time a "product" appears which, together with the predispositional energy, is sufficient to cause such an alteration, in the interaction of the corpuscular and cellular metabolism, as to lead to erratic growth on the part of those cells.

This would account for the structure of the secondary growths being of the same nature as that of the primary, as it only is between these cells and the corpuscles that the metabolic relationship is disturbed.

If, on the other hand, the interaction of the corpuscular and cellular metabolism is so affected as to cause proliferation of, or, in other words, an increase of the corpuscles, we get a condition known as leucocythæmia, or lymphadenoma.

Reverting, for a moment, to the theory suggested as to the cause of menstruation, it can be easily understood, on that theory, how it is that anæmic girls menstruate irregularly, or not at all, for in them we find the blood in such a poor state, *i. e.* with such a feeble corpuscular energy, that, barely sufficient to carry out the more common vital processes, it is quite unable to respond to the metabolic stimulus of the erupted ovum.

An erratic, or exaggerated, display of energy on the part of the corpuscles, at the expense of other parts of the body, as shown by emaciation or other signs, may cause menorrhagia.

In syphilis, the energy of the corpuscles being interfered with, it is easy to understand how, if pregnancy supervene, that energy which responds to the stimulus, due to the spermatozoon entrance, is unable to carry the proliferation of the endometrium (*i. e.* the deciduum) much beyond the normal monthly period, in other words, a *miscarriage* is produced.

Applying my definition of heredity to the case of congenital syphilis, it is easy to understand how, having a special breed of corpuscles transmitted from the mother to the infant, with their special predisposition, the corpuscles proceed to follow out the same course in the infant—modified, of course, by the fact that the corpuscles and body cells are in a state of evolution, or development—as did their ancestors in the body of the mother.

Here, then, is an example of an acquired characteristic, or habit (no doubt only a temporary one) being transmitted from parent to child, and the lesson it teaches us is, that for such an acquired characteristic to be so transmitted, it must be one in the production of which the whole energy of the corpuscles is affected.

As an explanation of Colles' law, I venture to suggest the following. When the father is infected with syphilis, the energy with which his spermatozoon infuses the ovum, is modified by the predispositional energy of syphilis, the result being the development of a breed of corpuscles which produce the characteristic signs and symptoms of congenital syphilis in the fœtus.

The metabolism of these corpuscles being thus affected, a product of the nature of an antitoxin is formed, which renders the mother immune.

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A CLINICAL LECTURE ON ASCITES.

Delivered at St. George's Hospital, July 2nd, 1900,
By H. D. ROLLESTON, M.A., M.D., F.R.C.P.,
Physician to the Hospital.

As there are a number of cases of ascites in the hospital under my care, I propose to make that condition the subject of to-day's clinical lecture.

First of all with regard to the word "ascites." It is derived from the Greek word *ἀσκός*, meaning a wine-skin; ascites, or in its full form, *ἡ ἀσκήσις νόσος*, is literally the wine-skin disease, *i. e.* that in which the abdomen resembles a skin distended with wine. The adjective "ites" [*ιτης*], also seen in tympanites, is much the same as the termination -itis (*ιτις*)—*e. g.* in iritis and peritonitis—and is a feminine adjective termination agreeing with the word *νόσος* (disease) understood. It is interesting to note that the termination -itis, which now always implies "inflammation of," has only acquired this meaning secondarily because the disease *par excellence* of various organs is inflammatory.

The different forms of ascites may be considered under two heads: (1) according to the different kinds of fluid; or (2) in connection with the causes of ascites. The latter is perhaps the most practical and convenient way, since of course ascites is only a symptom of some underlying condition which must be correctly diagnosed in order to be satisfactorily treated.

The different kinds of ascitic fluid.

(1) *Serous ascitic fluid.*—The commonest form is that in which the fluid is clear, of a slight citron colour, but quite transparent, with a specific gravity of 1010—1016. Thus we are dealing with a fluid which is more like a transudation than an inflammatory exudation. This condition is sometimes spoken of under the name hydroperitoneum; this form is commonly seen in cirrhosis of the liver and in chronic peritonitis.

(2) *Turbid ascitic fluid.*—When inflammation

attacks the peritoneum the fluid contains leucocytes and more fibrin-forming factors. This turbid appearance may be seen in cases where previous tapplings have brought away clear fluid.

(3) *Chylous ascites*.—In rare instances the effusion into the peritoneum is exactly like milk. This milky aspect is very well shown in this specimen of pleural effusion drawn off from a little girl aged six years, under Dr. Owen's care; this remarkably realistic milky appearance may be due to the presence of chyle in the pleural effusion. A similar appearance in the peritoneal fluid may result from rupture of a thoracic duct or some of its branches, and is spoken of as a true chylous ascites.

(4) *Chyliform ascites*.—In some instances the turbidity of the effusion gives rise to an appearance like that of milk, and may be spoken of as chyliform ascites. In other words, the ascitic effusion is like that seen in cases where real chyle is mixed with the effusion.

In chyliform or adipose ascites microscopic examination shows the presence of globules of fat in the suspended cells, the explanation probably being that the leucocytes have undergone fatty degeneration. Chyliform ascites is sometimes seen in cases of new growth inside the peritoneum; it has been supposed that the presence of new growth inside the peritoneum produces a poison, and that this poison acts on the leucocytes in the ascitic exudation so as to induce fatty degeneration in them.

(5) *Milky non-fatty ascites*.—Another variety of milky effusion is met with occasionally in which no fat can be found in the fluid. Such cases are sometimes spoken of as milky or lactescent non-fatty effusion.

The cause of the lactescent appearance has been much discussed, and has been variously described as due (a) to some proteid body formed as the outcome of degenerative processes in the leucocytes suspended in the fluid, analogous to, but on different lines from, the fatty degeneration met with in chyliform or adipose ascites; (β) to a limpid condition of the blood-serum seen in some patients, and said to be associated with parenchymatous nephritis; and (γ) to the presence of lecithin in the fluid.

There are thus three forms of milky effusions: the true chylous, due to the escape of chyle;

the chyliform; and the milky non-fatty. The distinction between the first two is very difficult.

(6) *Hæmorrhagic ascites*.—Another condition, which is rare but very striking, is that in which there is blood in the ascitic fluid. When it is met with it is often due to new growth inside the peritoneum, and usually sarcoma, as might be expected from the hæmorrhagic nature of that growth. It is not often seen in connection with carcinoma. The rarity of disseminated sarcoma in the peritoneal cavity may be correlated with the fact that hæmorrhagic ascites is decidedly unfrequent.

Hæmorrhagic ascites is sometimes met with in cirrhosis of the liver, and may be due to traumatism taking place at the time that the effusion is tapped. Perhaps the needle damages the capsule of the liver, or pierces one of the dilated veins underneath the peritoneum. In other cases some of the vascular adhesions, commonly found in connection with a cirrhotic liver, break down, and in so doing rupture the blood-vessels which run in them, so that the blood gradually oozes into the peritoneum.

The admixture of blood with ascitic fluid does not appear to be due to tuberculous infection of the peritoneum, as is sometimes the case with hæmorrhagic pleural effusions. In those very rare cases where the fluid is almost entirely blood, the term hæmoperitoneum may be applied. As a pathological curiosity reference may be made to cases in which the peritoneum fills up time after time with fluid which is practically entirely blood, without any satisfactory cause such as new growth or traumatism. Such a condition, however, does not come under the same heading as hæmorrhagic ascites.

Causes of ascites.

We will now consider the different causes of ascites. Those that are commoner and therefore more important than the others will be considered first.

Ascites due to—

1. Cirrhosis.
2. Chronic peritonitis of various kinds.
3. Cardiac and pericardiac disease.
4. Peritonitis, subacute or acute.

1. *Cirrhosis*.—There are two well-marked and fairly distinct kinds of cirrhosis of the liver. In

one of these, hypertrophic biliary cirrhosis, associated with jaundice and a large, smooth liver and an enlarged spleen, ascites either does not occur or is only seen very late in the course of the disease, and then, as a rule, only to a slight degree. It may be mentioned, however, that sometimes after a patient has for years manifested the symptoms and signs of hypertrophic biliary cirrhosis, the clinical picture alters and becomes that of ordinary cirrhosis. This I believe to be due to the secondary development of multilobular cirrhosis on the top of the biliary (or monolobular) cirrhosis, the condition becoming one of mixed cirrhosis.

In portal or common cirrhosis, which is especially associated with alcoholic excess, ascites very commonly supervenes towards the end of the disease. In the cases which die of cirrhosis ascites is met with in about 80 per cent. of the cases, so it is a very common terminal event in that disease.

Now as to the mechanism by which cirrhosis gives rise to ascites. It is very often ascribed to the great obstruction to the passage of blood through the portal vein exerted by the cirrhotic state of the liver. But there are difficulties in the way of acceptance of this simple explanation. These difficulties are — (1) That the blood-pressure inside the portal vein is probably highest earlier in the disease, at the time that melæna and hæmatemesis take place, whereas ascites occurs late in the disease, and may come on rather suddenly or rapidly. Sometimes the fluid is poured out at the rate of a pint a day, and that in a patient who has very little fluid to spare in the body. (2) Another objection is that in many cases of cirrhosis of the liver the legs may become oedematous before the ascites appears. When the converse occurs it is quite easy to explain it mechanically by saying the ascites compresses the veins inside the abdomen. But when the legs become oedematous first, it rather points to another explanation of the production of ascites, namely, that it is due to the presence of a poison in the blood which has a kind of lymphagogue action, and which excites exudation from the blood-vessels of the peritoneum. (3) Another reason against ascites being simply due to the increased blood-pressure is, that if the portal vein is ligatured in animals,

it does not necessarily give rise to ascites; there are, indeed, some cases of slow obliteration of the portal vein in man in which ascites does not occur.

On the other hand, in considering the causation of ascites it is only right to mention that when the portal vein suddenly becomes thrombosed ascites rapidly results. Thrombosis of the portal vein usually occurs in association with cirrhosis of the liver. There are also cases of extensive secondary malignant infiltration of the liver,—for example, in melanotic sarcoma, where the liver is so infiltrated with growth, the capillaries being widely blocked with emboli of new growth, that the passage of blood through the liver is so greatly interfered with that ascites occurs. There are, therefore, reasons to think that in certain cases pressure of tumours on the portal vein or blocking of the portal vein or its intra-hepatic radicles may play a causal part in the production of ascites. These facts are rather in favour of the ascites of cirrhosis being due, at any rate in part, to increased blood-pressure in the portal vein.

The following explanation is probably the most satisfactory, viz. that the occurrence of ascites is not due solely to the increased blood-pressure inside the portal vein, but that it very largely depends on the presence of a poison which so alters the vessel walls in the peritoneum that fluid is poured out, and if there is increased blood-pressure that favours the rapid exudation of the ascitic fluid.

With regard to the clinical characters of ascites occurring in cirrhosis of the liver, I pointed out to you that it is a phenomenon which is terminal, or which occurs very late indeed in the life history of the disease. It is very rare for a patient with cirrhosis of the liver to require tapping more than twice. This forms a great contrast to what we see in chronic peritonitis, where the patient may require and survive frequent tapings, in extreme instances even a hundred times.

2. *Chronic peritonitis* is a very frequent cause of ascites; it will be convenient to consider in order the different kinds of chronic peritonitis:

A. Simple chronic peritonitis.

- (a) Associated with adherent pericardium.
- (b) Associated with arterio-sclerosis.

- B. Tuberculous peritonitis.
- C. Malignant peritonitis.
- D. Chronic peritonitis associated with the presence of innocent new growth.

(A) In *simple chronic peritonitis* the peritoneum is uniformly thickened and opaque; since it contains a good deal of scar-tissue, it tends to contract and to compress the viscera it surrounds. Sometimes it is especially marked over the surface of the liver and the condition is then often spoken of as *perihepatitis*. I may say that, clinically, this chronic *perihepatitis* is nearly always a part of chronic peritonitis, and that in the rare cases where *perihepatitis* or thickening of the capsule of the liver occurs without any affection of the peritoneum elsewhere, ascites is rarely seen. Simple chronic peritonitis, sometimes called *perivisceritis*, seems to be due to, or at any rate to be associated with, one of two conditions. The first associated condition is *adherent pericardium*. There are cases in which the adherent layers of the pericardium are very thick and in which the inflammation seems to spread through the diaphragm to the peritoneum, and by a process of extension or continuity, general thickening of the peritoneum results.

In the other class of cases of simple chronic peritonitis, the associated condition is *arterio-sclerosis*, or wide-spread *atheroma* of the vascular system. This very often shows itself largely by its results in the kidney, viz. by producing the familiar granular or *arterio-sclerotic kidney*. Taking a large number of cases of chronic peritonitis it will be found that the great majority of those which are not associated with *adherent pericardium* are correlated with the presence of *arterio-sclerosis* and *granular kidney*.

In cases of chronic peritonitis associated with *arterio-sclerosis* the effusion occurs again and again. The patient should be tapped whenever this distension of the abdomen gives rise to discomfort, but the tapping is a palliative and can in no way be regarded as a curative measure. The necessity for multiple tapplings is very characteristic of and forms a marked contrast to hepatic cirrhoses in which after one or two tapplings the ascites does not, as a rule, recur. The cirrhotic patient, however, is no better off on this account, and generally becomes drowsy

and passes on to a toxic state which is followed at no long interval by death.

(B) *Tuberculous peritonitis*.—There are other kinds of chronic peritonitis, and one of them—a very familiar form—is *tuberculous peritonitis*. Tuberculous affection of the peritoneum probably nearly always is secondary to the passage of tubercle bacilli through the walls of the intestine into the mesenteric and lymphatic vessels and glands; the peritoneum then becoming involved. It occurs most commonly in children. When it is met with in adults it is sometimes seen in women and then may have spread from the Fallopian tubes; this form of tuberculous peritonitis tends to be rather localised, and may limit itself to the lower half of the abdomen—*encysted peritonitis*. The whole of the peritoneal cavity is not involved, perhaps only the lower third or lower half, and in some cases the encystment of the ascites has been so marked that its real nature has been overlooked and it has been regarded as a cyst and sometimes described as a cyst derived from the remains of the allantois.

When tuberculous peritonitis occurs in men, a rare event, it is generally associated with *cirrhosis of the liver*. You cannot impress upon your minds too firmly that alcoholic excess disposes not only to the production of *cirrhosis of the liver* but also to the development of tubercle. There is no doubt, to take a common example, that pulmonary tuberculosis is much commoner in people who are exposed to alcohol than in people otherwise similarly situated who have not indulged in alcoholic excess. Chronic venous congestion of the peritoneum, which is associated with *cirrhosis of the liver*, appears really rather to predispose to tuberculous infection than to protect against it.

There are several types of tuberculous peritonitis. In some of them the onset is almost acute, and may possibly be due to the acute and wide-spread infection involved when a tuberculous lymphatic gland bursts into the peritoneal cavity. In other acute cases the explanation may be that the peritoneum is the site of a mixed infection, other micro-organisms besides the *bacillus tuberculosis* being present in the peritoneal cavity. There are some cases of tuberculous peritonitis in which the chief symptom is not ascites but *tyimpanites*.

In the commoner and more typical forms of tuberculous peritonitis there may be a very considerable quantity of fluid, which may very closely simulate ascites due to cirrhosis of the liver, while in other cases the most salient features are bands, adhesions, or enlarged tuberculous lymphatic glands palpable through the abdominal wall.

(C) *Malignant peritonitis.* — An important variety of chronic peritonitis is that associated with the presence of multiple nodules of malignant disease in the abdominal cavity. When malignant disease is generalised over the surface of the peritoneal cavity, as is often the case in malignant disease of the female genital organs, or of the stomach, a chronic irritative process is set up which results in ascites. This ascites, like the other forms of ascites due to chronic peritonitis, though in a less marked degree, tends to recur time after time. In some cases, where the growth is sarcomatous the fluid which is drawn off may contain blood; while in cases of carcinomatous infection the fluid may be milky or chyliform. Sometimes when carcinoma, the commoner form of intra-abdominal malignant disease, has undergone colloid degeneration, the fluid which is drawn off may be sufficiently thick and tenacious to block up the cannula. There is a woman in the hospital at the present time who has been tapped twice or three times and has definite evidence of malignant disease, viz. nodules, probably secondary, in the wall of the rectum, while a large tumour can also be felt below the umbilicus. The fluid drawn off from her abdomen is very tenacious, suggesting the possibility that not only is there colloid carcinoma of the peritoneum but that it has set up a certain amount of ascites, and that some of the colloid material has become mixed with the ascitic fluid.

The fluid drawn off from an ascitic abdomen frequently contains leucocytes and cells which are swollen up from imbibition. It is sometimes thought that malignant peritonitis may be diagnosed by examination of the cells in the fluid. Personally I do not think that this can be done with any degree of success; but it is only fair to say that some recent observers believe that in the ascitic fluid of malignant peritonitis more cells show mitoses than in simple or tuberculous

peritoneal effusions and that the mitoses in the former are atypical, and that by careful observation of these data a satisfactory diagnosis can often be made.

There is a rather rare form of malignant disease, namely, primary sarcoma of the mesenteric glands. When it occurs it sometimes gives rise to a very rapid development of ascites indeed. There was a boy aged eleven years under my care in the hospital in 1896, who, clinically, looked exactly like a case of tuberculous peritonitis; he had the wasted face and typical complexion associated with tubercle. It was said that his abdomen only began to swell a week before admission and that it did so very rapidly; he was tapped ten days after admission and six pints of turbid fluid removed. He died two days after being tapped, and it was found that he had not tuberculous peritonitis as I thought, but sarcoma of the mesenteric glands. The fluid drawn off did not show any tubercle bacilli; but a similar failure to find bacilli may be experienced in tuberculous peritonitis since the bacilli tend to stick to the serous surface and only to appear in the fluid in such small numbers as to escape microscopic examination. The tuberculous nature of such cases may, however, be demonstrated by inoculation experiments.

(D) *Chronic peritonitis associated with the presence of innocent new growths in the peritoneal cavity.*—In rare instances ovarian cysts or uterine fibromyomata are associated with ascites which is probably due to chronic irritation of the peritoneum set up by the growths.

A rare but very remarkable condition is that in which papillomatous disease of the ovaries sets up recurrent ascites; papillæ from the original growth may become widely implanted on the surface of the peritoneum and grow independently of the main tumour. They are implantation growths and not infiltrating or malignant formations. Such cases imitate chronic peritonitis; their detection depends on the ovarian growths being felt in the pelvis by vaginal or rectal examination, or on fragments of the growth coming away through the trocar when the abdomen is tapped. Dr. Pye-Smith has recorded ('Trans. Path. Soc.,' vol. xlv, p. iii) a remarkable example of this disease in a woman who was under his care for nearly nine years and was

tapped 299 times "without once complaining of her lot."

3. *Ascites due to cardiac disease.*—In cases of backward pressure due to mitral disease it is a general rule that the œdema begins first of all in the feet. But there are, however, some instances in which the œdema begins first inside the abdomen, and considerable ascites develops while there is comparatively little œdema of the feet. These cases may be spoken of as examples of cardiac ascites. It has been thought by a French observer that the occurrence of ascites in cases of cardiac disease without much œdema of the lower extremities depends upon a congenital condition of the hepatic veins, enabling the blood which regurgitates from the right side of the heart to enter into the hepatic veins more readily than into the other branches of the inferior vena cava, so that the stress of backward pressure falls chiefly on the liver. The truth of this explanation is doubtful; it is more probable that the dilated condition of the hepatic veins seen after death in such cases is secondary and dependent on the backward pressure than that it is congenital.

Backward pressure due to heart disease most commonly shows itself by œdema of the legs, but by no means always. There are three main tributaries of the inferior vena cava, viz. the iliac, hepatic, and renal veins, and though the iliac veins generally suffer the most, the other two are sometimes more prominently affected. I have seen cases of mitral disease where albuminuria was so constant as to make one believe that there was primary renal disease, but in which microscopic examination of the kidneys after death only showed the effects of chronic venous engorgement. In other instances the brunt of backward pressure falls on the hepatic veins and the liver; phenomena such as enlargement, etc., are the prominent features. Such a condition has been called hepatic asystole and may be mistaken for cirrhosis of the liver.

Ascites due to hepatic pseudo-cirrhosis of pericardial origin.—In some cases of ascites the associated condition is, not mitral regurgitation and tricuspid regurgitation, but adherent pericardium. These cases have sometimes been spoken of as hepatic pseudo-cirrhosis, and they have especially been described by a German

authority, Pick. As the result of pericarditis in these cases, the inflammatory process spreads to the hepatic veins during the acute stage and weakens the walls of the hepatic veins so that they become permanently dilated. This enables the blood to regurgitate more readily into the hepatic veins than into the other branches of the inferior vena cava.

There is no universal chronic peritonitis or perihepatitis, and these cases are distinct from the condition described above of chronic peritonitis associated with adherent or calcified pericardium. I have, however, in cases of pseudo-cirrhosis seen an opacity of the surface of the liver which at first was described as perihepatitis, but was shown microscopically to be subcapsular cirrhosis, the capsule itself not being thickened.

As the result of long-continued and extreme chronic venous congestion, the liver is necessarily badly nourished and the hepatic cells undergo atrophy, so that the supporting fibrous tissue of the organ becomes more evident. This explains the term pseudo-cirrhosis. There is a certain apparent increase in the amount of fibrous tissue visible (replacement fibrosis of Adami), but this appearance is largely due to the shrinking of the more noble and highly functional part of the liver, viz. its cells, and not to the same active hyperplasia of fibrous tissue seen in ordinary cirrhosis.

4. *Acute and subacute peritonitis.*—Lastly, we ought to mention as causes of ascites, though it does not practically appeal to us in that way, cases of acute or subacute inflammation of the peritoneum. In all cases where there is acute inflammation of the peritoneum there is so much shock and the symptoms are so acute that we naturally and rightly pay practically little or no attention to the presence of ascites. Still, from an academic point of view, acute or subacute inflammation of the peritoneum is a cause of exudation of fluid into the peritoneum; the fluid contains a large amount of fibrin or lymph.

Results of ascites.

Having reviewed the different kinds and causes of ascites, a few words may now be said about the results of the condition. The results are chiefly due to pressure. The pressure of fluid inside the peritoneal cavity gives rise to a cer-

tain amount of pain, as you can imagine from the tension and stretching of the abdominal walls, but the patients do not often complain of it. The muscles of the abdominal wall, after having been repeatedly stretched, lose their tone and become very flaccid. The skin suffers and you get a rupture of the deeper layers of the skin giving rise to *lineæ albicantes*, familiar appearances in women who have been pregnant as *lineæ gravidarum*. It is interesting to note in passing that *lineæ albicantes* are not invariably due to mechanical distension, they may be trophic and occur after severe illnesses. I have seen them on the shoulders in pulmonary tuberculosis. Osler mentions their appearance after fevers, such as scarlet fever, and states they may be idiopathic and that then they occur in men as frequently as in women.

The distension of the abdomen by ascites presses the diaphragm up and leads to collapse of the bases of the lungs. A very good indication as to the performance of paracentesis may be obtained from seeing whether the patient is short of breath or not. When ascites has pressed the diaphragm up considerably, collapse of the bases of the lungs, especially of the right, may take place. Sometimes the resulting congestion of the collapsed lung tissue is so extreme that hæmoptysis occurs; the occurrence of hæmoptysis is therefore an immediate indication for relieving the tension caused by the fluid. The fluid also pushes the heart up and presses on the intestines and the stomach, and to a certain extent impairs their function. When the pressure of fluid inside the abdomen is considerable, the renal veins, which are much thinner than the renal arteries, may be compressed. This interferes with the return of blood from the kidneys and so gives rise to chronic venous congestion of the kidneys. This again impedes the passage of arterial blood into the kidney, and if the kidney is not properly nourished with arterial blood degeneration of the delicate epithelium which covers over the glomeruli tuft follows, and, as a direct result of that, albuminuria. Probably, in a certain number of these cases, albuminuria is produced in this mechanical way. In some other cases, the same poison which gives rise to the ascites may act on the kidney and cause toxic albuminuria. This

accounts for some of the cases of albuminuria occurring in cirrhosis of the liver. While again in chronic peritonitis associated with arteriosclerosis albuminuria may depend on granular kidneys.

The *treatment* of ascites is conveniently divided into two. (A) The radical treatment, which is sometimes possible, or removing the cause of the ascites. Now, in some cases generally diagnosed as cirrhosis of the liver the ascites disappears after treatment with iodide of potassium, and does not recur. It is certainly possible that some of these cases are due to cirrhosis, but probably, in the majority, this diagnosis is wrong though the treatment is right, and the condition is in reality syphilitic disease of the liver. It may not be possible to diagnose during life between cirrhosis and syphilitic disease of the liver, and therefore in all cases of ascites which may be due to cirrhosis it is well to give iodide of potassium on the chance that one's diagnosis is wrong and that the iodide of potassium may cure the patient by removing the cause, viz. such as the pressure of a gumma on the portal vein. Another radical mode of treatment is that often very successfully employed in cases of chronic tuberculous peritonitis, viz. simple laparotomy. When tubercle is scattered over the peritoneum its vitality seems to be so comparatively feeble that if the abdomen is opened and air admitted the life of these tubercles comes to an end. In the first case in which this was done it was the result of a fortunate error in diagnosis. A lady was diagnosed as having an ovarian cyst and the abdomen was accordingly opened for its removal. The operator found to his horror that she had tuberculous peritonitis and the abdomen was therefore quickly closed. But from that time the patient improved and lived for a long time afterwards. It has been thought recently that oxygen is the best thing to let in, and that it may do good, not only in tuberculous, but in other kinds of chronic peritonitis. At any rate, in whatever way it acts, operative treatment is followed by very successful results in a certain number of cases of tuberculous peritonitis. *Per contra* one must always remember that there are some cases of tuberculous peritonitis which gradually get well while they are lying in bed, being well fed, taking cod-liver oil, and having

mercurial ointment rubbed into the abdominal wall. In other words some cases of tubercle of the peritoneum get better if they are fed well and kept at rest, just as in pulmonary tuberculosis.

Now let us consider the treatment of ascites due to cirrhosis. Operative treatment has been employed for that, and since it was thought that the ascites was due to obstruction of the passage of blood through the liver, the treatment employed was to produce further vascular adhesions between the surfaces of the peritoneum, so that the number of anastomoses between the portal vein and the general systemic system was multiplied, and in that way the blood-pressure inside the portal vein diminished. That operation has been performed at least fourteen times, twice by Mr. Turner in this hospital. Of the fourteen cases seven have been reported as being very much improved; and as ascites in cirrhosis is generally a late event, occurring only two or three months before death, such results are encouraging. Of the two cases under Mr. Turner and myself, one was only slightly benefited, while the other was very markedly improved, though not cured. Some of the cases appear to have been really cured by that treatment. It is an interesting question as to how uniting the two opposed surfaces of the peritoneum and setting up vascular adhesions brings about any improvement. It is somewhat unlikely that it is due entirely to simply producing a fresh collateral circulation between the portal vein and the general systemic veins and thus relieving the blood-pressure and stagnation in the portal vein. It is perhaps more probable that the way it does good is by removing some of the extra stress of blood from the liver and so enabling the liver to deal more satisfactorily with the blood which does come through it. The production of fresh vascular adhesions may supply the surface of the liver with an increased blood supply and so give rise to a better condition and vitality of the cells so that they may proliferate and compensate for those cells which have been destroyed.

Now as to the palliative treatment. Formerly, paracentesis of the abdomen was put off as long as possible, because the peritoneum sometimes became secondarily infected as the result of paracentesis; acute peritonitis was set up, and thus

the patient died somewhat before his time. Bad results invariably follow deferring paracentesis as long as possible. At the present time, with antiseptic precautions, paracentesis should be performed as soon as there is inconvenience. The tapping should be done with a Southey's trocar, so that the fluid takes several hours to come away. After the paracentesis the abdomen should be strapped; this gives support to the flaccid abdominal walls, and I think that to some extent it prevents the reaccumulation of fluid and troublesome tympanites.

By the administration of diuretics and purgatives an attempt is usually made to remove the fluid from the peritoneal cavity. Suitable diuretics may be given, but care must be taken not to purge the patient excessively and thus weaken him. By these means fair results may be obtained, but by no means constantly. When any discomfort from ascites arises paracentesis should be performed.

Diagnosis of ascites from other conditions.

I will now mention very briefly some of the conditions which may give rise to difficulty in the diagnosis of ascites.

One of these conditions is the presence of a very large cyst inside the peritoneal cavity. The commonest of those is a very large ovarian cyst. It rises up from the pelvis, so that it may be first noticed as a tumour there. It distends the abdomen, first of all below the umbilicus, so that the measurement between the umbilicus and the pubes is bigger in the cases of an ovarian cyst than under other conditions. It may also displace the umbilicus laterally. An ovarian cyst grows up towards the anterior abdominal wall, which is dull while the flanks are resonant.

A very rare condition which has been known to simulate ascites is a very large hepatic abscess: this could only occur where an hepatic abscess is so large as nearly to fill the abdomen. Another condition is a very large hydatid cyst. A very large and distended urinary bladder has been known to give much the same impression. It is reported that John Hunter once tapped a urinary bladder as a case of ascites. Then, again, extreme obesity of the abdominal wall may sometimes simulate ascites; indeed, in fat people it is often very difficult to be certain

whether there is fluid inside the peritoneal cavity or not. Extreme œdema of the abdominal wall may give rise to the same difficulty.

Another very rare but interesting condition is a very large fatty tumour or lipoma inside the abdomen. Those are cases in which a fatty tumour grows either from the fat around the kidney or from behind the peritoneum; it forms a slow growing tumour which eventually may weigh as much as forty pounds, the fat being more or less fluid at the temperature of the body gives rise to fluctuation. These cases have frequently been tapped under the idea that they were ascites. As no fluid is removed the case is sometimes looked on as malignant disease, especially as the rest of the patient is emaciated. The consequence is that nothing is done, and then, after death, an enormous fatty tumour which might perhaps have been successfully removed is found. In 42 cases collected by Adami, the fatty tumour was removed wholly or partially in 26; of these patients 12 recovered. The fatty tumour, from its retro-peritoneal position, carries the intestine in front of it. Removal of the tumour may, therefore, deprive a considerable extent of the intestine of its blood-supply.

A large hydro- or pyonephrosis is so unilateral that it should not be in danger of being mistaken for ascites.

In rare instances doubt has arisen when the real condition was a dilated stomach, a greatly dilated gall-bladder, and a pregnant uterus with hydramnios.

Intubation and Tracheotomy in Cases of Croup Secondary to Measles.—M. Netter from his hospital experience is decidedly of the opinion that tracheotomy gives better results than intubation, the latter almost invariably leading to ulcerations and predisposing to broncho-pneumonia. It is not, however, to be altogether abandoned, but may often be used to advantage in the pre-eruptive stage of measles, at which time the accidents are usually spasmodic and do not involve the mucous membrane. In tardy croup, when the measles may be considered as quite cured, it is also frequently valuable. But when grave laryngeal accidents, occurring after the appearance and during the first two weeks, necessitate intervention, this should be tracheotomy.—*Amer. Journ. Obstet.*, June, 1900.

A CLINICAL LECTURE ON TWO CASES OF APPENDICITIS.

Delivered at St. George's Hospital, March 6th, 1900,

By A. MARMADUKE SHEILD, M.B., F.R.C.S.

GENTLEMEN,—This afternoon I am going to call your attention to two cases of appendicitis which have recently been in the hospital. They present some remarkable features, principally for the reason that they were operated upon very soon after admission, far more early and quickly than perhaps is usually the case. I will commence by briefly giving you an outline of the particulars of them.

The first of the patients was a schoolgirl, æt. 16, who had been in good health until January 29th, when in the middle of the day she was suddenly seized with very acute pain referred to the "pit of the stomach." I wish to call your special attention to this; the pain was referred by the patient to the middle of the stomach. This is common in appendicitis, and often leads to errors in diagnosis. At the same time she was twice very sick. Later on the patient describes the pain as "settling down on the right side." She was very sick again, and could not keep anything down. As usual, a dose of castor oil was administered by a nurse, but it was fortunately vomited. The bowels had acted once. Menstruation was present, and naturally enough the nurse supposed that this had to do with the symptoms present. She was brought to the hospital on the following day, that is to say, January 30th, and was at once operated upon for these reasons:—her pulse was exceedingly quick, nearly 120 per minute; there was a good deal of rigidity, resistance, and tenderness in the right iliac fossa, but no "lump" indicating a barrier of plastic lymph could be felt. The abdomen on that side was swollen, and meteorism was evidently coming on. The temperature was only 100°. A very important condition of things was discovered on making a small incision into the abdomen, one and a half to two inches in length. An inflamed swollen appendix was at once found, lying free among the intestines on the inner side of the cæcum. It was the size of one's finger, club-shaped, intensely red, and covered with yellow sticky lymph. On lifting the appendix out and attempting to ligature in the usual way it burst,

and there issued forth several drachms of virulent yellow pus, which we caught hastily on a sponge. The appendix was indeed only just removed in time. If a few more hours had passed we can hardly doubt but that the abscess would have given way into the general abdominal cavity, and that most fatal condition known as septic or perforative peritonitis would have ensued, and would have hurried her to destruction. The intestines were sponged and cleaned, and the treatment afterwards was the usual one,—that is to say, the bowels were immediately opened by a saline purge, turpentine enemata were given *per rectum*, and as soon as flatus passed freely we began to give some fluid food. For the first twenty-four hours nothing but tepid water was given by the mouth, and nutrient enemata every three hours by the rectum. She made an easy recovery.

The second case was that of a man *æt.* 27. He gave this history. A month before admission he had what he called an attack of colic; I wish you to particularly notice this. Attacks of what is termed colic are often enough really unrecognised appendicitis of a slight nature. The patient on this occasion felt rather uneasy, was given some opium pills, and got better. He remembers, however, the important fact that the pain was in the right iliac region. The day before he was admitted into this hospital he had a second attack of pain and tenderness, referred markedly to the right iliac region. Swelling and resistance on pressure was noted in the right iliac fossa. He had been sick once, and felt very nauseated. The doctor who saw him very wisely indeed advised surgical consultation, but the patient being a dispenser, and having, as he believed, a considerable knowledge of medicine, had taken opium freely. Now here follows a consequence I wish you most carefully to note. This man when he was seen by us looked comfortable, and said he felt very much better, "much better than on the day before." His tongue was foul and furred, his temperature over 100°, and there was marked tenderness on right iliac pressure. No "lump" was to be felt with certainty, but he had also the same important symptom which we noticed in the last case—a quick pulse. In the morning, carefully counted by the house surgeon, Mr. Pettinger, it was 80 to the minute, and in the afternoon 120, so that it increased very rapidly and progressively. On this

ground mainly, the rapid increase of pulse rate, I thought it better to explore the appendix. As usual with a patient enjoying the fallacious ease given by opium some objections were offered, but overcome by strongly expressed opinions on the perils attending delay. On making the usual small incision I found a very disastrous condition of affairs. The appendix was gangrenous and sloughy, pus was escaping through a hole in it. Round the sloughing appendix the peritoneum was intensely red and inflamed. Some soft flimsy adhesions had already formed, and it seemed as though a large abscess would probably have ensued, if not general peritonitis. All this mischief you will recollect occurred in about twenty-four hours, and it was on the occasion of that "second attack," which those who promulgate "rules" in the treatment of a malady having such protean characters as appendicitis tell us we should wait for before operating. The appendix was removed, the parts sponged, and this patient also was given calomel, salines, and turpentine enemata to get the bowels to act rapidly. On the second day he had some fluid diet by the mouth, and, as usual in cases treated by these measures, got quite well.

These two cases, operated upon on the second day of the attack, form a useful illustration of the very important subject of acute appendicitis, of which you know a large number of cases are operated upon in this hospital, and which will constantly come before you in your future practice.

The three varieties of appendicitis which are generally clinically recognised are—(1) so-called simple or catarrhal appendicitis, (2) the ulcerative variety of appendicitis, and (3) that variety where the appendix is gangrenous and ulcerated and a diffuse suppurative septic peritonitis occurs. This latter variety is well termed "fulminating," and is one of the most perilous affections human beings are subject to. The so-called simple appendicitis, as recently shown by Mr. Lockwood in his paper before the Medical Society, is almost invariably due to the invasion of the lymphatics of the appendix with bacteria. And the concretions found in diseased appendices are largely composed of faecal matter and organisms. Why the organisms pass through the mucosa is not quite clear; but when this happens an attack of inflammation takes place, which passes off perhaps to be repeated by another one. This view of the pathology seems

far more rational than to call these cases "gouty" or "rheumatic"! Recollect, then, that in the simplest case of so-called appendicitis there are all the potential factors present for a very serious disaster, if the inflammatory process should spread or be severe. You can never tell what the next attack will bring forth, and this is one of the dangers attending the practice of sending such cases to drink medicinal waters at a "Continental Spa"!

Now I will try to sketch out the usual history of a simple case of appendicitis. Of course the onset is sudden, the patient gets a certain amount of pain, which is often enough referred to the pit of the stomach, and is called an attack of colic. He feels sick and disinclined for food, and perhaps may actually vomit. The temperature rises to 100° or 101° , in a child it may be higher. The pulse is increased in frequency, and the quicker the pulse and the more its rate is increased the more reason there is for anxiety. On examining the abdomen you find some marked tenderness on pressure in the right iliac fossa, and an obscure swelling may also be detected, owing to the quantity of plastic lymph which is thrown out round the inflamed appendix. The presence of a swelling indicates that some sort of barrier is being formed to general peritonitis, and is not a sign of evil omen. The bowels are almost always constipated, and the patient remains in this condition for a variable time, from twenty-four hours to two or three days. The temperature then falls, the rapidity of the pulse also lessens, and the patient almost invariably gets well. This is a summary of the sort of case which it is estimated forms 60 to 70 per cent. of the whole. The vast majority of such illnesses, if treated in the way I shall relate to you, get temporarily well. Now when you meet with a case where the symptoms are comparatively mild, how are you to manage it? The common plan adopted and advised by many physicians is to give these patients opium. My own idea is that there is no medicine that will benefit these cases in any real remediable way, and I think that far and away the wisest thing is to leave the patient alone and watch the case. If opium be given at all, a suppository of one sixth of a grain of morphia can be administered, but this is only to relieve discomfort, nothing else; and I would even avoid this if I could. It is also good

practice to empty the lower bowel by enemata and thus allow some flatus to pass. Give small quantities of cold fluid diet and watch the temperature, and especially the pulse rate, with the most zealous care. If the case is going to turn out badly, there will be none of the fallacious improvements of opium to complicate your judgment. Locally, leeches and the ice-bag each have their votaries. I remember the late Dr. Hare used especially to dwell upon the efficacy of free leeching. Both these remedies may relieve pain, and there is no objection to them. But as a general rule, if you leave these cases alone and let them rest in bed, the attack spontaneously subsides. Perhaps the patient may go on without a second attack for many years; or he may have another quite soon, in a few weeks, and this may be a very serious one. One of the difficulties of the minor cases of appendicitis is the question of possibility of recurrence. All is uncertainty. You do not know when or whether the patient will have a return of his troubles, or whether, indeed, the next attack may not be fatal. So as long as the patient has a diseased appendix inside him, which gives rise to recurrent attacks, he is in a dangerous state. You may well compare such cases clinically to instances of neglected perforative otorrhœa. At any time, often most unexpectedly, a fatal blaze of inflammation may arise, and the disaster is often the more distressing because it is so unexpected.

Seldom will private patients allow operation in mild attacks of recurrent appendicitis. An attack severe enough to cause pain or alarm to the patient and his friends is often needed to convince them of the potential source of evil which is present.

Operation in mild appendicitis is usually done as a matter of choice, not at the time of the attack, but between the seizures. I think that all people who appreciate modern surgery will advise a patient who has such attacks, especially if he has had more than two, to have his appendix removed. The risk of removing an appendix properly through a small incision is practically *nil*. I have done a considerable number of these cases, and never have a moment's anxiety about them, and I do not see why any case should go wrong with ordinary care. Where many attacks have existed the extent and nature of the adhesions may severely tax the skill

of the most experienced operator. I ask you to remember this, and never undertake one of these cases with the idea it is going to be easy or simple. The second class of case, where you get ulceration of the appendix with perforation and abscess, is in its symptoms much more severe than the last. The onset of the attack is more alarming, and so is the pain. Very severe pain is commonly associated with a correspondingly severe lesion internally. This is, I think, as true as such matters can be, and is of *much clinical importance*. The patient may cry out, and commonly lies in bed with his leg drawn up, so as to relax the fascia and peritoneum over the inflamed appendix. Vomiting is more urgent, the temperature higher, and the pulse is apt to be more rapid and to progress more quickly than in the last variety. So you see these two classes of case only differ in degree, and every gradation may be noted between them. You find a large exquisitely tender mass in the iliac fossa, which rapidly forms, and which you can readily feel, and the difficulty which arises is to know when or whether pus has formed. Now the signs of the formation of pus in these cases are generally as follows:—The swelling, instead of getting smaller and retrogressing towards the third day, persists or gets larger. The mere persistence of one of these swellings beyond the third day of an attack generally means the formation of pus. Pus may be detected in these cases by rectal examinations. You should always examine *per rectum*, and by this means you will sometimes feel a sensation of elasticity or actual fluctuation. The pulse rate—and this is an especially important matter—progressively increases. The patient often sweats profusely. If you have a mass in the right iliac fossa persisting over the third day, and the pulse mounts up to 105, 110, 120, in such cases I always advise operation. I simply tell you to-day what I would do personally, because there are great differences of opinion regarding the treatment of these cases, and we shall hardly ever be quite agreed about them.

A word *apropos* of the temperature in appendix abscess. It is very extraordinary what peculiarities of temperature one gets in abscess associated with disease of the appendix. Temperature may be normal, or may be even subnormal in acute attacks. I have known large abscesses filling up the iliac fossa associated with a nearly normal temperature.

this reason the temperature is not so good a

guide in these cases as the pulse; but sometimes these patients have a succession of rigors and sweats, and then one begins to suspect, with good reason, that there is infection of the portal vein, and that the patient has got, in fact, portal pyæmia with abscesses in the liver. This is one of the worst complications of appendix abscess that can happen. *So although absence of high fever does not exclude abscess, the presence of it is an additional evidence of the existence of pus.*

I may add here that pus in the appendix may be the cause of unexplained sepsis, with daily rigors and sweats. It is a region of the body to be carefully examined in doubtful cases of pyæmia. As a rule, a case that sets in with very severe symptoms—bad pain, vomiting, and so on—will generally terminate in abscess or peritonitis. My own practice in these cases, I may say at once, is to operate very early. If I am called to a case of appendicitis, and the symptoms come on with marked and alarming severity, I always explore the appendix. The only mistake I could make is to forestall an abscess or general peritonitis, which I have not the least objection in doing. I may say at once that even the most severe cases *may* be brought to a successful issue by giving opium and leaving them alone. An abscess may burst into the bowel and the patient recover. This treatment I would never sanction, and the responsibility must rest with the doctor or relatives who insist upon it.

The third class of case is that known as gangrenous appendicitis with ulceration, or fulminating appendicitis. I think this disease furnishes some of the most deceptive and tragic cases which you will hereafter meet with, and I therefore wish to speak very anxiously about it. First of all, these cases are unfortunately almost invariably mistaken for acute intestinal obstruction, which, indeed, they very closely resemble. In several instances I have known the onset to occur during severe exercise—as football,—or to be attributed to a blow. A patient is smitten with sudden acute abdominal pain referred to the pit of the stomach. He vomits most urgently. The first vomit is green fluid, and it will go on to become brownish and foul. The belly rapidly swells, with rigidity and tenderness, and the bowels are absolutely constipated. The pulse at once becomes remarkably rapid. The temperature may rise at first, but it soon falls as the strength of the patient ebbs. The purga-

tives, which are unfortunately too often given in these cases, are providentially vomited. Within twenty-four or thirty-six hours symptoms appear which may first show the unwary what he has to deal with. The extremities become cold, the face dusky and pinched and anxious, with deep hollows under the eyes. The pulse, which at first is very rapid and hard, becomes in twenty-four hours to two days so fast and feeble that you can hardly count it at the wrist. The belly is greatly distended and very tender, and all food is vomited. The mind of the already moribund man is clear and collected. He often cannot realise he is severely ill, and if he has been given opium he "gives the lie to death" by declaring himself better. In these most dreadful cases speedy operation is the only hope for life, and the prognosis is in direct proportion to the time that has been lost and the extent of the septic peritoneal infection. When the "collapse" symptoms have set in with a weak and rising pulse, recovery is hardly to be hoped for. These cases are peculiarly dangerous and deceptive in children. I have seen several instances in young children, and I cannot help believing that they are commonly overlooked. The child has agonising abdominal pain, vomits, and its belly begins mysteriously to swell, with a rapid pulse and some fever. I may remark that an additional diagnostic point about these acute cases is that you may sometimes detect fluid in the cavity of Douglas's pouch. This is not invariably the case, for the intestines may be glued together by yellow sticky lymph. In other instances the belly is full of thin evil-smelling pus, which may be detected by rectal examination.

The diagnosis of these cases from acute intestinal obstruction, often difficult, is made in the following way:—Vermicular movements of the intestines, marked in acute intestinal obstruction, are absent in acute perforative peritonitis. In acute intestinal obstruction there is not necessarily fever, nor intense pain and tenderness on pressure over the right iliac region, nor an extremely rapid pulse. If the latter symptoms are present you can be pretty sure that the case you have to deal with is one of acute fulminating appendicitis. In cases of doubt, remember that an inch and a half incision in the right iliac region will at once clear up the matter. The appearance of healthy intestine negatives perforative appendicitis. If these cases are

treated, as they often are, by opium and "expectancy," the patient will slip through your fingers and *pass quietly into the realms of hopelessness almost before you realise what has happened.* These cases, especially occurring in young people, and possibly in the only child of an important family, are apt to become terrible disasters both to the patients and to your own reputation. If you carry out the rule which I told you of just now, and remember to operate at once if the symptoms are very severe, or if you do not operate yourself, call somebody in to see the case with you who can operate, you will escape these dangers and share a very weighty responsibility with others. Another common error is to treat the acute peritonitis and to ignore its cause. Acute peritonitis is due in nine cases out of ten to perforations of some hollow viscus, or the bursting of an abscess. Thus, perforations of the appendix, stomach, duodenum, or abscess of the Fallopian tube take a leading part in its production. Such lesions can only be dealt with surgically, and can only be dealt with successfully before the patient is poisoned by the septic products of his own peritoneum.

Next I think I may say a few words as a general summary of my views regarding the giving of opium in these cases. It is all very well for us in hospital circles to preach against its use. I think you will find when you get into practice that you will be driven, against your will, to give opium in bad cases of appendicitis. The pain is so great, and the patient's distress and misery so marked, that you must do something to relieve him. If driven to administer the drug, give a small dose of opium, that is to say a quarter to half a grain by the mouth, or, what is better, one sixth to one eighth grain of a suppository of morphia by the rectum. You will find that even such small doses will give very marked relief to the symptoms; but note the condition of the appendix is in no way altered, and because the patient feels and says he is better, do not be deceived as to the necessity for operation. If the pain again increases, and the patient craves for more opium, it is an additional proof for the necessity of operation. In these cases it is no exaggeration to say opium may completely relieve the patient and abolish all fear on the part of the doctor if given in large and continuous doses. The patient remains perfectly quiet, his countenance becomes more tranquil, he says he feels

relieved, and he knows he is going to get better. An experienced surgeon feels the pulse, but to his dismay can hardly count it at the wrist. He finds the abdomen swollen and tympanitic, and he knows that fatal mischief has progressed unchecked, and that the signs of it have been absolutely concealed by the opium which has been administered. On the third or fourth day of such an illness the patient's friends will send round and say that a "sudden change" has taken place in the patient, and would the doctor come round to see him? And now alarm for the first time is created. The patient is dying, though he does not know it. But there is a marked alteration in his face; the eyes are sunken, the extremities are cold, the pulse can no longer be felt at the wrist, and in a couple of hours the patient is dead. Having seen several of these distressing cases, you will realise how strongly I feel that the giving of opium in appendicitis should be limited to a small dose at the outset of an acute attack in order merely to give the patient relief from his pain. Symptoms severe enough to call for more opium should be regarded as usually indications for operation.

Next let us devote a few words to the treatment of the so-called mild cases of appendicitis, which I spoke of at first. I told you that the best plan in such cases at first was practically to do nothing but leave them alone, except for the emptying of the rectum by enemata, and giving some small quantity of fluid nourishment by the mouth. The next point concerns the treatment of these cases between the attacks. A large number of patients who have recurrent appendicitis of a mild type suffer from digestive disturbance, and particularly chronic constipation. A large proportion of these cases of appendicitis are found in constipated individuals. Therefore you have to carefully diet them for constipation, and a course of mercurial purgation in small doses is often beneficial. Massage of the abdomen must, of course, be negatived. It may do much harm. Having got the intestinal canal to act regularly, occasional small doses of salol should be given for several weeks. If the patient has bad teeth, and cannot masticate his food, send him to the dentist to see that the teeth are put in good order. It often happens in cases of recurrent appendicitis, that by regulating the bowels and diet no further attacks take place. If they do occur it is far wiser to have the appendix

removed. Another point often needs consideration. If the patient is going to live abroad, or in an out-of-the-way part of the country away from skilled aid, he would be well advised to have his appendix removed at once if it is unsound. I have already pointed out to you the extreme uncertainty of these cases, and how impossible it is to judge whether the very next attack may not be of a most dangerous nature. Here it would be well if I were to say a few words as to how you ought to operate upon an abscess in connection with the appendix, because it is an operation which you are sure to have to do, and it is not one of great difficulty. Make a curved incision close above Poupart's ligament, and carry it through the muscles one by one, and then you come down upon the thickened adherent transversalis fascia and peritoneum, through which you pass your finger into the abscess, and out will come some stinking offensive pus. You may wash the abscess cavity out with hot sterilised water, or wipe it with pledgets of gauze. I prefer the latter method. The question then will arise, are you to remove the appendix or not? There are great differences of opinion with regard to this. My own rule is to always remove the appendix if I can at all readily get at it. I sweep my finger round the cavity, and if I feel the appendix, and can shell it out, I remove it. I am sure in the majority of cases of abscess the appendix ought to be taken out at the time of opening. Some of these patients, however, are too ill to bear prolonged manipulations, and there is a risk of breaking down the cavity and infecting the general peritoneum.

Very frequently the abscess spontaneously cures the appendicitis by the appendix sloughing off into the abscess cavity, and coming away in the discharge. But this is by no means universal. I operated the other day upon a gentleman who—about three years ago I treated for appendicular abscess. He had then an enormous abscess burrowing nearly to his diaphragm above, and below as far as the pelvis, and a large amount of offensive pus came away. He was so desperately ill that one could not think of removing his appendix. Tubes were put in, and the abscess flushed out, and he was carefully nursed, and although extremely weak he made a good recovery. A sinus remained for a long time, but that ulti-

mately closed, and he was congratulated on his recovery, and I informed him, as one naturally does inform such persons, that he would probably get no further trouble, as the appendix was destroyed by suppuration and sloughing. He went on for all this time without a symptom. He played golf, and led an active life. But about a month ago he got another attack of very acute inflammation, and the question was whether he should be operated upon. He had a tender mass in the right iliac fossa, which was carefully watched. The pulse did not mount to any serious extent, the temperature on the third day subsided, and the swelling gradually disappeared. After this I urged the removal of the appendix, which I felt sure was there still, and ultimately the operation was done. The appendix was found matted in a lot of adhesions behind the cæcum, thickened, inflamed, and puckered from various perforations. He will now remain quite well. This case illustrates what I am continually harping on, that you do not know what is going on in tissues and organs; you cannot see and inspect, and if an appendix is once diseased, there is no safety or peace for the individual until he is able to see it in a bottle.

Probably medical men will never quite agree on any routine treatment of appendicitis, because what one may consider a severe case another may consider mild, and *vice versâ*. But I am as confident as I can be that the safe plan in the treatment of this most protean malady, is to explore the appendix in cases which are ushered in and accompanied by symptoms of any degree of severity. You never can tell whether an abscess is going to form, or whether there is going to be fulminating peritonitis, or what is going to happen. The only possible objection to operation in such cases is to prevent the formation of an abscess or peritonitis.

In conclusion I want to say a few words about the operation for removal of the appendix. The proceeding for removal of the appendix is, as a rule, a very simple one. On the other hand it may be very difficult, or indeed impossible. Provided the patient has not had many attacks—that is to say if you remove it after the first or second inflammation, it generally comes out with remarkable ease. The appendix should be removed through an incision of not more than one

and a half to two inches in length. In such cases it is well to split the muscles in directions parallel to their fibres; I believe this lessens the tendency to hernia. The finger passed down into the abdomen will often at once come upon the enlarged appendix, which can be readily brought out. If you have no inflammatory lump to guide you, and you cannot feel the appendix, you should put your finger in and pull out a coil of large intestine, which is recognised by the longitudinal bands in the surface. You follow these down over the cæcum, and the appendix is then generally found. A common place for the organ is behind the cæcum tucked up and pointing towards the kidney. But it may be located anywhere about the cæcum. It may be outside it, behind it, or pointing towards the umbilicus, or even hanging down in the pelvis. Sometimes it is of extraordinary length, as you see in this specimen. In a case like this the appendix may come down into Douglas's pouch or enter a hernia. I have known a concretion and abscess in the appendix to be felt through the vagina and be mistaken for a suppurating ovary. I may say that when difficulty occurs from numbers of dense adhesions, it is wiser to make a large incision at once, so that you can readily manipulate and expose the cæcum and surrounding parts. Much of the delay witnessed in these cases is due to the surgeon trying to remove through a little opening an appendix concealed and matted down by dense adhesions.

The actual amputation of an appendix should be done in the following way. If it is exceedingly sloughy and bad, as is often the case, you tie it and cut it off, using for the ligature soft silk of moderate thickness carefully boiled. Here there is one point to which I must call your attention closely, because disasters have occurred from neglect of a very simple precaution. The appendix has a mesentery, and in this, close to the appendix, lies a little vessel. It is a terminal twig of the ileo-colic artery, which runs along the root of this mesentery, and branches out over the appendix. If you merely tie the appendix and snip the mesentery across, you may leave the artery bleeding into the peritoneal cavity. You hardly notice it at the time, and yet the patient may subsequently have serious intra-peritoneal hæmorrhage. Employ the trans-

fixion method of ligature, or use the "Staffordshire knot," so that slipping is out of the question.

Supposing that the condition of the appendix is not so bad, that it is not inflamed, and not ulcerative or gangrenous, the usual way of dividing it, and perhaps the best, is as follows:—First tie the mesentery and its artery with a separate piece of silk thread; secondly, remove the appendix by making a circular incision round it, and turn down a "cuff" of peritoneum. Cut off the appendix, and ligature it with a fine piece of soft silk, then turn your "cuff" of peritoneum up, and sew it together over the stump, in much the same way as you amputate an arm, supposing the bone to represent the lumen of the appendix, and the soft parts the peritoneum. I disinfect the stump in all cases with pure carbolic acid. If the appendix has sloughed away from the cæcum, the opening into the cut should be touched with carbolic acid, inverted, and united with a Lembert suture. Regarding the after-treatment of these cases, recollect that the more severe the case—the more peritonitis there is—the more reason there is for getting the bowels to act. If you get an early action of the bowels and passage of flatus in a bad case of appendicitis operation, the patient will almost invariably get well. The purgative I usually give is two or three grains of calomel, followed by sulphate of magnesia and syrup of figs, combined with a carminative, such as ginger and peppermint-water. After that, two or three hours, a turpentine enema of the pharmacopœial strength is administered. I have even gone on giving compound jalap powder and elaterium rather than let the patient die with distended bowels full of pernicious material. It is surprising how many of these patients can be brought round by purgation. Some of them die simply because the surgeon has not courage to get the bowels to act. In most cases, however, calomel and salines will do all that is needful. I avoid morphia after operation if possible.

As regards dietetic treatment, these patients after operation are thirsty. I see no objection to let them sip small quantities of tepid water. Nourishment during the first twenty-four hours should be in the form of rectal enemata, which may consist of beef-tea and small quantities of brandy, with eggs beaten up with them. The more fluid the enema, and the less solids it contains the better.

In operations for recurrent appendicitis, where the condition of the patient is generally good, the usual treatment is to get the bowels open on the third day, and a simple aperient with an enema generally suffices.

These are some of the points, but only some, regarding a most intricate subject, the treatment of appendicitis. It is very difficult to know exactly how to deal with these cases, and when you get into practice you will find your difficulty enormously enhanced. I can only say that a patient who is brought into a London hospital has a far better chance of escaping with his life than the millionaire who dwells in a palatial mansion in Belgravia. You will find that the higher the social status of the patient the less chance he generally has. Morphia holds its benign sway. Early operation is scouted or refused by the relatives or medical men. Even the resources of homœopathy are invoked in deference to the wishes of some important member of the family. When the surgeon is at last allowed to operate the case is well-nigh hopeless, and his operation is blamed for the death, which is really due to culpable delay. I take up a strong line in these circumstances, and advise operation if the symptoms are severe. If the relations do not agree to this, I tell them that I am perfectly willing for anyone to take charge of the case who will take the responsibility of "haphazard" treatment. The case may recover without operation, but the risks run are very serious. I trust that in your future practice, when, as assuredly will happen, these anxious cases come before you, you will remember my words. The necessity for operation is in proportion to the severity of the initial symptoms, and especially their progression from bad to worse. Acute peritonitis is practically always due to an infective focus, which needs early surgical treatment. The concealment of acute abdominal symptoms by morphia is a plan of practice fraught with risk.

Spina Bifida Lumbalis.—Krause demonstrated a boy, two years old, who had been operated upon for spina bifida shortly after birth. Soon after the wound had healed hydrocephalus developed, and had increased to an enormous size. In addition there were paraplegia of both legs and bladder and rectum paralysis. Krause thinks that the injection of Lugol's solution is the method most to be recommended.—*Munch. Med. Woch.*, No. 16.

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A CLINICAL LECTURE

ON

INTUSSUSCEPTION.

Delivered at St. Bartholomew's Hospital,

By W. J. WALSHAM, M.B., C.M., F.R.C.S.,
Surgeon and Lecturer on Surgery to the Hospital; Con-
sulting Surgeon to the Metropolitan Hospital, Bromley
Cottage Hospital, and Hospital for Hip Diseases,
Sevenoaks.

GENTLEMEN,—I have within the last few years had some five or six cases of intussusception under my care in the wards of this hospital, and I have been fortunate enough to have four of these recover after abdominal section. I have chosen this subject for our clinical lecture to-day because I am anxious to impress upon you the importance of early diagnosis and prompt treatment. In the case of strangulated hernia I suppose no one would be found now to hesitate, if taxis failed after a few minutes' trial, to at once operate or send the patient to the hospital for operation. But it was very different in the case of hernia some twenty-five years ago. The sort of letter one used not infrequently to get then was as follows:—"Will you admit this patient, who I believe is suffering from strangulated hernia? I have applied taxis two or three times a day for the last week, and although I have used considerable force I have been unable to get it back. I have told the patient's friends, therefore, that I think it has now become a hospital case." And then we used sometimes to have a postscript, "I hardly think it is of any use to try purgatives, as I have given almost every purgative in the pharmacopœia without effect." One of the first cases I had as surgeon at the Metropolitan Hospital came in with some such a history and letter, and I well remember how my first incision opened a large faecal abscess filled with castor oil and *débris* of calomel pills; in fact, the whole of Scarpa's triangle was found pretty well dissected out by the destruction of the infiltrated connective tissue. Now, how-

ever, it is very different. The modern strangulated hernia letter runs more in this way:—"The hernia only came down early this morning. Taxis has been applied for a few minutes, but as the hernia will not go back, will you take the case in?" I operate, as you know, at once, and as a rule—I may say almost invariably—the patient recovers without a bad symptom: and so with intussusception. Here also the importance of early interference is evidently becoming recognised, for on reference to the hospital statistics we find that from the year 1870 up to the year 1890 the average day of the disease on which these cases were sent to the hospital was the fourth, whilst from 1890 to 1897 half the cases admitted—and they numbered forty—were sent in on the first day of the disease. So it is beginning to be recognised by those who see these cases first how important it is that treatment should be undertaken at once, before the invaginated intestine has been injured by the constriction, or rendered irreducible by adhesions.

Before reading the notes of my cases let me briefly call your attention to a few points in the anatomy of intussusception, for which purpose I have had some specimens brought from the museum and placed on the table. As shown in these specimens, an intussusception consists of an invagination of a portion of intestine into the intestine below. The invaginated portion is called the intussusceptum, the part which is invaginated the intussusciens or sheath. The intussusceptum consists of two portions of intestine doubled on each other, the innermost portions being called the entering tube, the other portion the receding, inverted, or returning tube. Drawn in with the intussusceptum, between the entering and the receding tube, is the corresponding portion of the mesentery, and you will see on looking at this specimen that in consequence of the dragging of this drawn-in mesentery the invaginated intestine lies within the sheath in a curved manner, so that the aperture leading into the entering tube (or upper portion of the intestine) instead of being centrally placed is drawn to one side, and instead of remaining a small round opening becomes slit shaped. In the mesentery there is often a large lymphatic gland or two, or rather a lymphatic gland or glands, which have become enlarged and congested from the compression. The swelling of

these glands may render an intussusception irreducible.

As a rule it is the upper portion of the intestine which is invaginated into the lower. But I have on the table a specimen in which the lower portion of the intestine is invaginated into the upper. In this specimen also you see a multiple intussusception. These are rare conditions, but it is well to remind you that they do exist.

Where a piece of small intestine is simply carried into small intestine the intussusception is known as the *enteric* variety, and is the simplest of all. Sometimes instead of the small intestine it is the colon which is invaginated into the colon. This is called the *colic* form, and also is a simple variety. The commonest situation of an intussusception, however, is at the ileo-colic junction, and there we have two varieties, one called ileo-cæcal, and the other ileo-colic. The terms are unfortunate, and a little confusing. In both the ileum passes into the colon, but in the ileo-cæcal form it is the ileum *with* the cæcum which is carried into the colon, whereas in the ileo-colic it is the ileum which has passed through the ileo-cæcal valve into the colon. All my cases were ileo-cæcal; the ileo-colic form is very rare. Now you may ask, how can you tell the difference? Having opened the abdomen, when you look at the ileo-cæcal variety you find simply a smooth rounded portion of large intestine with a piece of small intestine passing into it; you can see no vermiform appendix at all. But if it is the ileo-colic form, you see the appendix attached to the cæcum.

I have said that the commonest variety of all is the ileo-cæcal. Mr. Treves makes it out to occur in 44 per cent of all cases, but I think he has under-estimated it; I am sure it is very much more common than 44 per cent. The hospital records make it out to be as much as 92 per cent. of all cases. Mr. Treves gives the ileo-colic form to be 8 per cent. of the total cases, but I believe it is not so common as that; our hospital statistics make it work out at $1\frac{1}{2}$ per cent. The enteric, according to Treves, is 38 per cent., according to the hospital it is $4\frac{1}{2}$ per cent.; the colic according to Treves 18 per cent., according to our hospital records $1\frac{1}{2}$ per cent. Of course you do not need to commit to memory these percentages. All I want you to remember is that the ileo-cæcal is common, and consists of the ileum with the cæcum invaginated

into the colon, and that the ileo-colic, in which the ileum is carried through the ileo-cæcal valve into the colon, is extremely rare. Also that in the ileo-cæcal the appendix vermiformis is invaginated, and is not seen until reduction. Therefore you should not be satisfied, if you are dealing with the ileo-cæcal form, until you have seen the appendix actually come into view and resume its normal relations to the cæcum.

Now a few words about the pathological changes that occur. The gut is nipped by the part where it passes in, called the collar or neck, and congestion is set up, resulting first in œdema of the sub-mucous tissue, and afterwards in paralysis of the muscular coat; then inflammation, and finally either gangrene or ulceration of the nipped part sets in, much in the same way as happens in strangulated hernia. It is the very rarest thing for the intussusceptum to slough off and be passed *per rectum*. Here, however, is a specimen in which this happened, but it is the only case of the kind which occurred in the hospital from the year 1870 to 1897, that is to say, once in sixty-eight patients. Therefore we cannot trust to this fortunate termination.

I will now refer to my own cases. The first was that of a female child three years of age, who was admitted last April. She was quite well up to April 23rd, but on that date was seized with a sudden abdominal pain. She passed a small motion, and vomited after this every half hour. The pain was paroxysmal. On April 24th the vomiting ceased, and the mother gave her some castor oil, which was followed by a slimy motion in the afternoon, but the pain continued at intervals by day and by night. On April 25th the condition was much the same. The child looked ill, had a dry tongue; pulse 140, feeble; temperature 101.2°. The abdomen moved on respiration, was not distended, and was resonant. The pain came on in paroxysms, especially during examination. No tumour could be felt until an anæsthetic was given, when a distinct sausage-shaped swelling could be felt tucked under the right ribs. No blood was passed *per rectum*.

The other cases were very similar, therefore I will not trouble you with the details of each. All six patients except one—an adult woman—were young children of three years of age or under. One was aged three, one eight months, one two

and a half years, one four months, one six months, and one (on whom I operated for Sir Thomas Smith) was under two years. I wish to impress upon you the fact that intussusception is a disease of infancy and early childhood. I have only seen two adult patients suffering from the condition myself, and you will do well to bear in mind that 75 per cent. of the cases admitted to this hospital were under one year of age. The youngest patient was only three days old. Most of my patients were boys, and it is found to be more common in boys than in girls. I do not quite know why this should be, but I suppose one reason is that boys are apt to get phimosis or a small meatus, and consequently there is need for more straining. You may ask, Why is intussusception more common in children than in adults? There are certain reasons which go some way to account for it. In the first place, the mesentery is much longer in children than in adults, and so may more readily allow invagination to occur. Another reason given is that immediately after birth the colon rapidly increases in size, whereas the ileum does not increase at the same rate. So that it comes to pass that soon after birth there is present a very big colon, with a very small ileum opening into it; and consequently all the conditions favourable for the invagination of the smaller into the larger tube. Further, it has been shown by Nothnagel and others that if the small intestine is made to contract artificially by irritating the muscular coat with chloride of barium, that the peristalsis stops when it reaches the ileo-cæcal valve, whilst a fresh set of peristalsis occurs in the muscular fibres of the colon. So it may readily happen, especially during the irregular and violent peristalsis that may be set up by such irritants as castor oil or injudicious diet, that the ileum may be completely contracted at the same moment that the colon may be dilated to its greatest extent, and so the ileum may get a little bit tucked in to the colon. Then the colon contracts and violently seizes it, and so “swallows” it, as it is called, in just such manner as a portion of food is seized by the œsophagus after it has passed through the pharynx.

Next a few words about the symptoms. You know that there are several cardinal symptoms of intussusception, and every one of you would, I have no doubt, mention them if asked. They are—abdominal pain, vomiting, the passage of slime

and blood-stained mucus, and the presence of a sausage-shaped tumour, which can be felt either by the rectum or through the abdominal parietes. I want to say a little about each of those cardinal signs.

You do not always get all of them present in each individual case. Thus in my first case, I have told you there was no blood passed whatever.

Abdominal pain is always present, but it is, perhaps, the least important of the signs I have mentioned. Still, the pain has some characters to which you should pay attention, as they will help you to distinguish it from the pain of colic. Of course colicky pain in infants is very common; indeed, it is so common that mothers often take little or no notice of it, simply giving a little dill water or the like. The pain of intussusception comes on suddenly. Secondly, it is intermittent. One moment you see the child looking comfortable, but if you examine its abdomen the next moment it screams out with pain. It is a peculiar scream of sudden pain, the pain being clearly intense.

In the first case, of which I have read you an abstract, when the child came into my room there seemed to be little the matter with her till I proceeded to examine her abdomen; and when she was first seen in the ward she looked so well that my house surgeon and others were doubtful whether this could be the patient I had telephoned to him to admit as an urgent case of intussusception. But on making an abdominal examination the characteristic pain came on. This intermittent character of the pain is due, no doubt, to the fact that on manipulation peristalsis is set up.

The next symptom is vomiting. This by itself is, of course, not of much significance, since it may occur from many other causes, but when of sudden onset and combined with intermittent attacks of pain, it should lead us to make inquiries as to what are the conditions of the motions, and to carefully palpate the abdomen. A very much more important sign is the passage of bloody mucus, which is characteristic. This, of course, comes from the intussusceptum. It is squeezed, becomes congested, and the over-full vessels force out this bloody mucus, which escapes *per anum*. As I have said, this is a very important sign, since it occurred in no less than fifty-six out of the sixty-eight cases I

have mentioned. You must not confound the escape of blood in this condition with that which occurs sometimes in flatulent colic in babies. A baby with flatulent colic may have a hard motion, and pass a little blood by the anus, just as may occur in piles, due to congestion of the mucous membrane about the anus. Therefore you should always see the motion yourself, so that you may judge whether it really has the characteristics of the bloody slime which attends intussusception. When once you have seen the latter you will not readily mistake it for anything else.

The last symptom I want to refer to is the presence of a tumour. This may be felt in the rectum, or it may be felt merely through the abdominal walls. A tumour was present in 83 per cent. of the cases admitted to this hospital. Therefore the passage of bloody slime and the presence of tumour are by far the most important signs. When a tumour is present, it most often occurs on the left side, though you might have thought it would be more frequently on the right, seeing that the cæcum is on the right. Fortunately for the examination, the abdomen is, as a rule, lax, certainly not tense, whereas in colic it is tense and rigid. Of course if the intussusception has been followed by rupture of the gut, and peritonitis has been set up, the abdomen will be tense, but that does not apply to early cases. Sometimes the tumour may be felt transversely, running across the abdomen in the direction of the transverse colon, and in the case which I have related to you in detail it was found tucked under the right false ribs. When this child came to my consulting room I felt the tumour distinctly, and the child cried a little with pain as I felt it. But when she was admitted to the hospital it was not discoverable. It had got under the ribs, and neither my house surgeon, Mr. Wood, Mr. D'Arcy Power, nor Mr. Eccles, who happened to be in the ward, were able to detect it. As soon, however, as the patient was under the influence of an anæsthetic, there was no question about it whatever. Therefore I hold it to be very important in making an early diagnosis to find out whether there is a tumour in the abdomen, and if there is any doubt I strongly advise you to examine the patient under an anæsthetic. Then the chances are that by a little careful manipulation on the relaxed abdomen you will discover the swelling. The tumour has well-

marked characters, as you would expect; it is sausage-shaped, it is in the course of the ascending transverse or descending colon, and when you handle it you can often feel that it moves and gets harder under the manipulation. In a child hardly any swelling but an intussusception would give these signs. Of course in an adult the tumour might be impacted fæces, but I am not speaking of adults now.

Another symptom called the *signe de Dance* may at times be present, and it is this. If you find a tumour on the left side, or in the course of the transverse colon, and press on the right iliac fossa, you may feel a want of resistance there, as if something was absent at that spot. Such a lack of the normal resistance is owing to the cæcum being carried over to the left side. I do not think this sign is of very much value.

As to treatment, my own mind is very clearly made up, namely, that we ought to open the abdomen at once. You will say that is not quite orthodox. I know it is not. The majority perhaps of physicians, and some surgeons, would say that you ought, under ordinary circumstances, to try what injections will do first. Some would recommend inflation, but not many, I think. As far as I can learn, no case has been cured in this hospital by inflation. It is, in my opinion, a dangerous method, since it may cause splitting of the colon, inasmuch as you cannot properly estimate the amount of force that may be safely applied. Moreover, one has not always the apparatus at hand to carry out inflation. Therefore we will leave inflation out of the question. Some surgeons still recommend as a first measure, injection. Indeed, some hold that it should be used two or three times; whilst a few, even up to a year or two ago, taught that if you do not cure the patient by injections you had better leave him alone, since, until quite recently, the results of abdominal section for intussusception, save in exceptional cases, have been so disastrous. I think hardly any surgeon at the present day would hesitate to open the abdomen in an early case after injections had been tried and failed. Therefore we may take it that many physicians and some surgeons still recommend injection.

My objection to the treatment by injection is that it is unreliable and wastes time. In my

experience, although it is occasionally successful, it more often fails, and if it fails precious time is lost, since every hour the intussusception remains unrelieved the swelling and congestion increase, the walls of the gut are further damaged, and adhesions, if already formed, become firmer, so that on subsequently opening the abdomen it may be found impossible to reduce the intussusception, or during the reduction so much damage may unavoidably be done to the bowel, that a successful termination can hardly be hoped for. A second objection is that the method is deceptive. Many cases have been recorded, in which it is said the intussusception has returned after complete reduction by injections. My belief is that in these cases the reduction was not complete. Some years ago I was asked to see a case in which it was stated that such a reduction by injection had been effected, but that the symptoms had remained unreduced, and the intussusception had quickly returned. On opening the abdomen I found that the bowel could be easily pressed out from the sheath to within the last inch or so, but beyond this it was not possible to reduce it without splitting the peritoneal coat. I have met with several other cases at the hospital, where after the abdomen had been opened, the last inch or so could not be reduced. What happens in these so-called cases of return of the intussusception after cure by injection, I believe, is this:—that as the injection forces out the intussusception from its sheath to near the ileo-colic junction, the tumour is felt by the surgeon to gradually recede along the course of the colon; and the portion that remains unreduced, being small, is in consequence not felt through the abdominal parietes, and it is imagined that complete reduction has been effected.

A third objection is that the method is not unattended with danger. The injection may very readily cause a splitting of the peritoneal coat of the colon. It has been shown by experiment that two and a half pounds of pressure on the square inch, which is the pressure produced by holding the injecting tin five feet above the patient, may cause splitting of the serous layer of the colon, and that when the tin is held eight feet above the patient it always produces this result. In some cases, in which the tin was only held six feet above the patient, it caused actual rupture of the bowel. Moreover, the size of the colon and its distensibility,

vary greatly in children of the same age. In a child of five months it has been found that ten ounces completely distended it; whereas in another child, only two months older, the colon would easily hold thirty ounces. There is great uncertainty, therefore, as to what amount of fluid the colon will hold without injurious distension. Moreover, it must not be forgotten that after the intussusception has existed some hours, the coats of the bowel are apt to become softened, and therefore more liable to rupture than in the healthy gut.

Lastly, there is the fact that when using injections you are working in the dark. You cannot see what you are doing, and cannot tell with what form of intussusception you are dealing, or what may be the conditions of the parts. The enteric form could hardly be reduced by injection without dilating the ileo-cæcal valve, and to do this, pressure, dangerous to the walls of the colon to say the least, would have to be used. I imagine, therefore, that no one would employ injections if they were certain that they were dealing with the enteric form.

However, many cases have been cured by injections, and notwithstanding the reasons that deter me from employing it myself, some of you may feel differently, and I believe that the weight of authority is against me. Therefore, if you do try injections, I advise you to take the following precautions.

(1) Always give chloroform; (2) do not raise the injecting tin more than three feet high, above the patient; (3) use salt solution, at a temperature of about 100° ; (4) let the fluid go in *very* slowly, and whilst it is running in, place your hand on the abdomen, so that you can feel what is happening; (5) let the fluid remain in the bowel about ten minutes; (6) never vary the pressure, that is to say, do not let the tin run empty and then fill it up again, or vary the height that it is held above the patient; (7) do not use more than a quart of fluid. If you follow these rules in an early case you *may* have a success, but I think you are much more likely to have a success if you open the abdomen at once.

Of late years my rule of practice is to open the abdomen immediately the diagnosis is made. In all four cases now under review, and they occurred consecutively, this was done: and all recovered. I attribute the success to the early interference,

and the rapidity with which the operation was performed. The time occupied from the first incision till the closure of the wound, was respectively eighteen minutes, fifteen minutes, twenty minutes, and eighteen minutes. If you make an early diagnosis, and at once do a laparotomy, the intussusception can be reduced quickly, and consequently with little or no shock; and the patient, as in the above cases, is likely to recover without a bad symptom. Time will not permit me to discuss to-day, the best situation for the incision—which in passing, I may say, I strongly hold is the middle line,—with other points in the technique of the operation; what had best be done if the intussusception is found irreducible, and the after-treatment? I would, however, emphasize the importance of having the child well wrapped up, with the body as little exposed as possible, in the same way as you would do if you were opening the abdomen of an adult. Not merely to allow the child to be exposed, as I have seen before now, on the operating table, with practically nothing on below the diaphragm. These points I must reserve for another lecture.

A Case of Double Acute Retro-bulbar Neuritis, Hereditary in Origin.—By Dr. H. F. Hansell ('Amer. Journ. Oph.,' June, 1900).

—The case reported was a man, 56 years old. He had gone to bed one night feeling perfectly well and the next morning found himself almost blind. There was absolutely no cause that could be discovered. The ophthalmoscope revealed a low grade of optic neuritis with slight swelling (1 D.). There was a positive absolute central scotoma in each eye without contraction of the visual fields. Vision was $\frac{1}{200}$ (excentric) in each eye. Several of his aunts and uncles on his mother's side became nearly blind in adult life. One brother, a physician, had a sunstroke, and two years later was almost blind, and has continued so for twenty-five years. Another brother became suddenly blind at the age of fifty. From the data at his command Dr. Hansell infers that all these cases were acute double neuritis followed by atrophy. The author reviews briefly the literature, and mentions the different theories that have been applied to other cases. As to his own case, he says:—"The ætiology is obscure." The only plausible exciting cause was repeated exposure to cold during the six weeks preceding the attack.—*Post-Graduate*, August, 1900.

WITH DR. TATE IN THE OUT-PATIENT DEPARTMENT, ST. THOMAS'S HOSPITAL.

GENTLEMEN,—This patient is thirty years of age. She is a married woman, and has had one child, which was born three years ago. She has noticed that her abdomen has become larger ever since her confinement. She thinks the enlargement has been more rapid during the last six months. She now complains of pain in the left side of the abdomen, which has been getting worse lately. Menstruation has been regular every four weeks, and is scanty in amount. There has been no dysmenorrhœa, but there has been a slight vaginal discharge. There is no more uncertain symptom of which a woman complains than enlargement of the abdomen. Sometimes you find the enlargement is simply due to fat on the abdominal walls and in the omentum; sometimes it is due to the distension of the intestines with flatus; while in other cases you may find either a cystic swelling or a solid tumour, or free fluid in the peritoneal cavity. From the facts obtained in the history of this patient it is impossible to say whether we shall find any tumour, or whether the enlargement is due to some other condition. On inspection of the abdomen you see there is considerable distension, and the distension is accompanied by a certain amount of eversion of the umbilicus. As a rule, where a patient has a large deposit of fat in the abdominal walls the umbilicus is depressed. Of course you have to be prepared for the fact that there is a difference in the appearance of the umbilicus in different patients, and that previous distension of the abdomen by pregnancy or other tumour will cause eversion. Where the abdomen is enlarged owing to a deposit of fat or to tympanitic distension, if you get the patient to take a deep inspiration you find the respiratory movements can be seen over the whole of the abdomen right down to the pubes; whereas, if the enlargement is due to tumour, the respiratory movement is limited to the upper part of the abdomen, and the lower part occupied by the tumour hardly moves at all. You will notice in this case that when the patient breathes there is a fair amount of move-

ment over the whole surface. When one comes to palpate the abdomen you see the hand sinks in, and it is possible to press the fingers down almost to the promontory of the sacrum. Therefore the knowledge one gains by inspection in this case is confirmed by palpation. It is quite evident that the enlargement of the abdomen is due partly to the deposit of fat, but to a great extent to tympanitic distension of the bowels, with laxity of the abdominal wall. On a hurried inspection of the patient you might have thought there was a tumour of considerable size in the abdomen, but the abdomen is everywhere quite lax, and there is no evidence of any cystic swelling or any resistance in the deeper parts. It is probable that in this case there is about $1\frac{1}{2}$ inches of fat on the abdominal wall. Where there is a considerable deposit of fat on the abdominal wall you nearly always find a similar deposit of fat in the great omentum, which will also increase the enlargement. This is one of the conditions of the abdomen which is sometimes classed under "phantom tumour." The patient thinks she has got a tumour; but when you come to examine her you find the enlargement is due to flatulent distension of the intestine and to deposit of fat. That is the commonest form of phantom tumour which is met with in women of middle age. In young women or girls between eighteen and twenty years of age the most frequent kind of phantom tumour is that due to a large deposit of fat in the abdominal wall without any marked tympanites. The true "phantom tumour" is a tumour formed by contraction of the recti muscles, and is only occasionally met with. On vaginal examination there is laceration of the perineum in this patient; the cervix is looking downwards and backwards, and the uterus and ovaries may be felt in the normal position. There is no enlargement of the uterus, and the posterior parts of the pelvis are entirely free from any abnormal swelling. The uterus is freely mobile, showing that there is no inflammatory mischief around. This is a very easy case to examine and diagnose, because the abdominal walls are so lax. You sometimes find cases of enlargement of the abdomen due to tympanitic distension, in which the muscular walls of the abdomen are somewhat rigid, which gives the sensation of an encysted tumour. Some of these cases require very careful examination in order to arrive at a correct dia-

gnosis. Another point to remember is that in some of these patients with very thick adipose abdominal walls, and with a lot of fat in the omentum, there is impaired resonance on percussion. The impaired resonance may give rise to the diagnosis of a tumour being present.

We will now pass a sound to confirm the bimanual examination. In doing so one passes the finger up to the external os, and then directs the sound along the finger into the cervical canal. Then you usually find that by gentle pressure the sound passes inwards to the level of the internal os—that is to say, about one inch—without any difficulty. Difficulty may arise when you want to get it beyond the internal os. Where the canal is perfectly straight, and the internal os not unduly constricted, it passes in easily. But in other cases you may have to depress the handle of the sound where the uterus is anteverted, or to raise the handle where the uterus is retroverted, to allow the sound to pass up to the fundus. This patient appears to have been sent to the hospital because it was thought she had a tumour present. When she stands up you can see that the abdomen protrudes enormously—quite as much as if she were nine months pregnant. I shall advise her to wear an abdominal belt, for it is this kind of case where great relief is afforded by a support. The corsets which patients ordinarily wear push the enlargement down towards the pubes, which is just what one wants to avoid; whereas a properly made belt will give support in an upward and backward direction. At the same time it is well to give some medicine to relieve the amount of flatulent distension from which she suffers. The medicine I generally order for this kind of case is—bicarbonate of sodium, 15 grains; tincture of valerian, 20 minims; spirit of chloroform, 10 minims; compound infusion of gentian to 1 ounce. Tincture of valerian is a valuable drug to use where there is much flatulent distension. The distension is also diminished by the improved tone of the muscular wall of the bowel, as a result of the pressure exerted by the abdominal support.

The next patient is thirty years of age, and has had one child, the confinement having occurred five years ago. It is said that the confinement was followed by puerperal fever. Puerperal fever is a very vague term. All cases of puerperal fever are due to septic infection, which may be either a

chemical form of poison or a general septicæmic infection. If it is a chemical poison, due to retained products undergoing putrefaction, the symptoms are in some proportion to the dose of the poison, and after the decomposing material has been removed there is a reasonable chance of the patient making a good recovery. In the more severe cases of septic infection a large proportion of the cases end fatally within a week, though a few may recover. Where the patient has been ill for three or six months—as in the case before us—it is more likely that she has had some local inflammation following the confinement, either in the nature of pelvic cellulitis, or salpingitis with peritonitis. With this long history of illness, it is probable that the patient has some disease remaining behind of a chronic nature, of which we may find some evidence when we come to examine her. Her catamenia now are regular, sometimes painful; indeed, she has complained of pain in the abdomen since her last confinement. She has been quite relieved by wearing a ring, but the ring has been discarded for eighteen months. The pain has been getting worse for the last seven weeks, and has been accompanied by vesical trouble. She, however, has not been laid up in bed during the last eighteen months. She was better when she wore a pessary for a time. This is not the usual experience when some chronic form of peritonitis is present. The introduction of a ring or any form of pessary in a case where there is pelvic peritonitis usually causes increased pain; and sometimes the mere insertion of a support of this kind will determine an acute attack of inflammation. She tells us that she had pleurisy and pneumonia after her first confinement, so it is difficult to say what the exact nature of that illness was. On abdominal examination there is nothing abnormal to be felt.

Per vaginam one finds the uterus is retroverted, the cervix looking downwards and slightly forwards. On the left side one can feel the left ovary slightly enlarged and prolapsed; that is simply the result of the uterus lying back. If the ovaries and tubes are free from adhesive inflammation the ovary and tube must fall back with the uterus, so that they can more readily be felt than in the normal situation. The ovary is freely movable, and I do not think there will be any difficulty in replacing the uterus either with the sound or with the finger. There is no evidence of perito-

nititis around, nor any bands fixing the uterus down posteriorly. On the left side all you can feel is the right broad ligament. The left ovary is a little larger than normal, and very likely it contains a small cyst. Where you find an ovary enlarged and somewhat globular in shape, the enlargement is usually due to the presence of a small follicular cyst; these slight enlargements are of no significance. There is no marked tenderness on either side of the pelvis, so there is no evidence of chronic peritonitis. If you feel the ovary movable you may be fairly certain that the uterus is movable too. In nearly all cases where the uterus is bound down by adhesive inflammation you find that the uterine appendages take part in the adhesive peritonitis. Usually one finds that these cases are due to chronic salpingitis, or slight catarrhal salpingitis sealing the ends of the tubes, causing the uterus and ovaries and surrounding structures to be bound down together. When the uterus is retroverted the anterior fornix appears to be more shallow. If you want to replace a retroverted uterus, push up the body with the internal finger through the posterior fornix; then, after you have pushed it up as far as you can, you pass the fingers round to the anterior fornix, and by pressing the cervix backwards you lever the body forwards till it comes within reach of the external hand, which then completes the forward movement. If you cannot do it with the fingers, you have to use a sound. In this case there is very little tendency to retroflexion. When you bring the uterus forward in this way you bring forward the ovary as well. One always uses a Hodge pessary in any case of retroversion or retroflexion, as it gives very satisfactory results. The upper end lies in the posterior fornix, and the lower border behind the symphysis pubis. If you examine now, after I have put in the Hodge, you will feel the uterus is well anteverted. The Hodge is known to be acting satisfactorily only if you examine after it has been placed in position and find it well anteverted. Sometimes you may bring the uterus forward with a sound and put in a Hodge, and then you may find the uterus is bent back over the Hodge. That usually means there are some bands fixing the uterus, and after you remove the sound these bands drag the uterus back again. A Hodge pessary under these circumstances is no good. Unless you get the uterus well forward, and it remains anteverted, it is better to do without

a Hodge. It is always advisable for patients to use a vaginal douche once a day when wearing a support. It is, perhaps, not necessary to use it every day where a vulcanite instrument is inserted; but unless it is ordered for daily use, the patient is very likely to forget it altogether.

The next patient is thirty years of age, a married woman, who has never had any children. Menstruation has been regular, and she has lost more since she has been married. She had an attack of inflammation three weeks after her marriage, and was in bed for a month. She has had a burning pain on micturition during the last month. She had an abscess in the vagina three months after marriage, and a discharge. She has a slight discharge still. She had a little discharge before marriage. There are many things in this history which are suggestive. Her principal symptom is dyspareunia, which may be due to a variety of conditions. It may be due to an abnormal condition about the vulva, such as ulceration, caruncular conditions about the urethra, or malignant disease of the urethra, or of the vagina. You also get it in acute vaginitis, and in cases where there is no abnormal condition evident about the vagina at all, which we describe as cases of vaginismus; in these cases there is spasm of the muscles around the vaginal orifice due to some cause which we do not understand. A large number of cases of dyspareunia are not due to any abnormal condition about the vagina or vulva, but are due to some inflammatory disease or some other abnormal condition about the uterus and its appendages. One of the most frequent causes of dyspareunia is that condition where the uterus is retroverted and bound down by adhesive peritonitis. During coitus there is pressure on the uterus, causing dragging of adhesions, which gives rise to very severe pain. In any case where there is inflammation of suppurative disease of the Fallopian tube or ovaries, dyspareunia is almost always present. Occasionally you find it in unhealthy conditions of the cervix. One sometimes comes across cases of erosion or eversion of the cervix, in which the cervix is extremely tender. As far as I have seen carcinoma of the cervix is not a common cause of pain on coitus, though it causes hæmorrhage. We have an account here that three weeks after marriage she had some attack of inflammation, for which she was in bed a

month. These cases of inflammation following marriage are frequently due to salpingitis, the result of gonorrhœal infection conveyed along the uterine mucosa to the tubes. Ever since then this patient has had pain off and on, and discharge. She had "whites" before she was married. By "whites" one means a discharge somewhat milky in appearance, due to exudation from the vaginal wall and cervical glands. In some anæmic subjects it may be very abundant. The discharge contains a large number of vaginal epithelial cells. When you get an acute inflammation of the vagina, the discharge may become purulent from destruction of epithelium. In a large number of cases purulent discharge from the vagina is due to some form of endometritis, extending up from the cervix to the body of the uterus. In all probability the discharge that this patient has had for two years is due to endometritis of gonorrhœal origin. As she has had some discharge and pain on micturition for the past two years, and as she also suffers from some dyspareunia, it is not unlikely that we shall find some evidence of salpingitis, either mild or more serious. Apparently she has never been ill since the first attack. She thinks she is a little thinner than before. Cases of dyspareunia which one sees in hospital practice are more often due to some inflammatory condition in the pelvis than to disease about the vulva. A common cause of dyspareunia of vulvar origin is vascular caruncle. This patient gives a history of an abscess of the vagina, which she says burst. This may have been a suppurating Bartholin's cyst, which would still further point to the infection being of gonorrhœal origin. Suppuration of Bartholin's duct is one of the conditions you would look for as a result of gonococcal infection. This patient's cervix is looking downwards, and is lying far back in the pelvis. The body of the uterus can be felt acutely anteflexed. The cervix is movable, but the body is less so. On the left side the ovary can just be felt. On the right side there is a distinct resistance in the posterior portion of the pelvis in the situation of the uterine appendages. Another point is that directly one presses the cervix forwards the posterior surface is found to be bound down by bands to the posterior part of the pelvis. The thickening due to the bands goes outwards and backwards to the right side. That is very typical. She describes the pain as having

been more severe on the right side. The left side of the pelvis is perfectly free. You can feel the ovary far out in the posterior part of the pelvis, but there is no evidence of resistance on the left side. The cervix is movable, but the body slightly impaired in mobility. You can feel some bands passing outwards and backwards from the right side of the cervix, which are evidently due to matting of the appendages on that side, and probably a few adventitious bands, the result of an old adhesive peritonitis. Examination of the vulva does not cause any pain. There is no caruncle or acute vaginitis. The first point at which pain is produced is when one gets to the back part of the cervix and presses it forwards. We are therefore justified in coming to the conclusion that the inflammatory condition present is the cause of the dyspareunia. For treatment she should have rest in bed, and freedom from intercourse. Glycerine tampons introduced every other day, and removed the following day, may give relief. This treatment is of use where inflammation is subacute. This patient is an anæmic subject, and very likely general tonics will assist other means of treatment by improving her general health. Remember that dyspareunia is only a symptom, but in severe cases it may make married life unbearable, and one cannot ignore it. In such severe cases as these one can always offer a patient permanent cure by operative treatment, the object of the operation being to separate the adhesions, and, if necessary, remove any disease that is present. Where there is salpingitis on one side, even where there is not a great amount of thickening or disease, you very often find the tube on one side is sealed up by inflammation, though the mucous lining may not show any obvious disease.

The next patient is thirty-two years of age, and has had two children and three miscarriages: the last miscarriage is said to have occurred three weeks ago, since which time she has been in bed. She has had pain in the stomach for nearly a month, that is to say she had this pain for a week before the supposed miscarriage. She had been quite regular in menstruations up till November, so there is no history of amenorrhœa. All these cases of obscure miscarriages make you think of the possibility of extra-uterine gestation. As there is no history of amenorrhœa, we have no actual evidence of any pregnancy having occurred. Of

course the fact of the absence of amenorrhœa does not exclude the possibility of pregnancy. In many cases where hæmorrhage starts just after a period, very often that is due to slight separation of the ovum from the uterine wall, occurring in quite the early weeks of gestation, but it may also be one of the early symptoms of tubal gestation. Three years ago she was under my care for what she says were "abrasions," for which she was "cauterised." The patient has been getting very thin lately. She has had great pain in passing her motions lately; her urine is very thick and dark. There is a history of sore throat and hair falling out, but no offensive discharge. There has been no discharge lately, but there was a discharge of blood three weeks ago. She is very anæmic indeed, and looks very ill and feverish. From her aspect one would suspect some acute inflammation in the pelvis, or pelvic hæmatocele, the result of tubal abortion; or she may have some malignant disease in the pelvis or in the rectum. On examination, there is nothing to be seen abnormal about the breasts, which are quite flaccid. The abdominal wall is a little rigid, which is probably due to fear on her part rather than to any tenderness. There is no evidence of peritonitis on abdominal palpation. The vagina is a little pallid. The cervix is pushed upwards and a little to the right, and the body of the uterus is anteverted and the fundus displaced to the left. The uterus is oblique. All I can tell you here is that there is a swelling behind the uterus, evidently pushing the uterus a little forward; this swelling lies between the uterus and the hollow of the sacrum, and is of some considerable size. It does not depress Douglas's pouch to any great extent, though you can feel the resistance through Douglas's pouch. I cannot feel anything in the rectum itself. To make a complete examination of this patient without an anæsthetic is out of the question, because there is so much tenderness that a satisfactory bimanual examination cannot be made. One or two points are obvious. First of all there is a displacement of the uterus: where the uterus is either laterally displaced or very much pressed forwards, you may be almost certain that the displacement is due to the pressure of some tumour behind it or on one side of it, or to dragging of the uterus by adhesions on one or other side. Here the uterus is anteverted and also oblique,

and behind is a mass filling up the hollow of the sacrum. This mass is acutely tender: it can be felt through Douglas's pouch, extending to the pelvic wall on either side. It is evident that the mass is inflammatory, because there is an acute history—sudden pain at the onset of the attack—and she has been in more or less severe pain ever since. If it is inflammatory the actual nature of the mass becomes a question which you can only settle by examination under an anæsthetic. It may be simply an adhesive peritonitis, or it may be a mass of serous exudation in Douglas's pouch roofed over by adherent coils of intestine. In reference to that, the bulging down of Douglas's pouch is not sufficient to make one think there is any serous exudation in Douglas's pouch, nor is the swelling sufficiently elastic. The other things one has to think of are more or less mixed conditions; there may be diseased conditions of the Fallopian tube and of the ovaries as well. You may have a mass like this due to pyosalpinx, associated with an inflamed ovary—a peri-ovaritis—or you may have a salpingitis associated with an inflamed ovarian cyst, or with a suppurative condition. These masses lie in the posterior part of the pelvis, and on either side of the pelvis, they form fixed tumours in that situation, adhering to the back of the uterus and to the front of the posterior pelvic wall. The degree of fixation generally bears some proportion to the length of time they have existed, and also to the presence or absence of pus in them. If you have to deal with a pyosalpinx or suppurating ovarian cyst, you also almost invariably get a considerable amount of fixation, due to secondary involvement of the cellular tissue. If you have an inflamed ovarian cyst or hydrosalpinx, you may have fixation, but not complete; that is to say the whole roof of the vagina does not feel fixed and firm. Judging from the amount of fixation, it seems probable that there is inflammation of the pelvis, associated with some suppurative disease of the uterine appendages. An accurate diagnosis can, however, only be arrived at after careful examination under an anæsthetic. I think she will probably require an exploratory laparotomy. The question of operating by vaginal section, is a very important one at present, because it is being much discussed. Surgeons who treat these cases of inflamed tubes and ovaries, through the vagina, speak highly of the method. The

great advantage of operating through the vagina, is that you do not by that means interfere with the general peritoneal cavity. Free incisions should be made in every direction where collections of pus or fluid are felt, packing the cavities with iodoform gauze and allowing the parts to granulate up. This procedure causes very little shock, and it is supposed to yield good results. In this hospital we have not yet given up the abdominal method; it is one of those surgical procedures which are under trial. What we want to know is whether the ultimate results, in these cases where pus collections are evacuated, are as good as where the diseased appendages are removed. When you lay open the pus cavity, you still leave the ovary and tube, and in many cases you cannot be certain whether you have removed all the disease or collections of pus, and any focus of suppuration left behind may set up fresh inflammation. By the vaginal method of operation the mortality is markedly less, but whether the ultimate results are as good we do not yet know. It is surprising what an amount of manipulation patients will stand in vaginal operations without suffering appreciably from shock, provided care be taken to control hæmorrhage. In abdominal operations, if the operation is prolonged beyond an hour a certain amount of shock is nearly always present. Even after a comparatively simple operation by the abdomen the patient may suffer profoundly from shock. In this respect, therefore, the vaginal route certainly has the advantage.

The next patient is aged thirty-eight. She is married, and has had ten children; no miscarriages. Her last confinement was four months ago. She complains of dyspepsia. She has had severe pain on passing her water this last two months. No blood has been noticed in her urine. She has had occasional dyspareunia and incontinence for a week. The patient says her nerves are very weak, though she is well in herself. There has been no discharge. She has been nursing her baby, and has not seen anything since the birth of the child, except two small clots. It is a curious history, pain on passing water and dyspareunia at this stage. Of course she might have a urethral caruncle, or she may have cystitis. It is not very uncommon for a patient to have gonorrhœal infection following confinement; one sees many examples of it in the out-patient department. One has also to bear in

mind the possibility that she has again become pregnant, though that is not very likely. She might have a retroverted gravid uterus, which could account for the pain and difficulty in passing water, and also for the dyspareunia. There is still some pigmentation in the breast, but no pain in the abdomen. On examination of the abdomen, one can pass one's hand deeply down into the pelvis without feeling any evidence of inflammation. There is no caruncle, nor any abnormal condition about the vulva. The uterus is high up, almost out of reach, and lying back, and you will feel in front of the uterus a little cystic swelling, tense, and not bigger than a hen's egg. We must ascertain whether it is the bladder with some urine in, or whether it is a cyst in the vaginal roof. It is quite likely it may be nothing more than a little urine in the bladder. It is, however, a little further back on the front of the uterus than is usually the case where distension of the bladder is present. I will ask you to pass a catheter while I feel the swelling. If it is a new growth, it is probably a cyst in the vaginal roof; it is unlikely to be an ovarian cyst, owing to its situation. A vaginal cyst in this situation might cause a little pressure on the bladder, and thus cause frequency of micturition. Sometimes collections of fluids in the pelvis are due to a little encysted perimetritis, or thin-walled broad ligament cysts. I have found such cysts disappear during examination. One can feel the swelling in this patient disappear while the catheter is being passed. The patient says sometimes her water comes freely, but not at other times; this morning the attempt to pass water caused her so much pain that she could not do it. The catheter passes without any difficulty, but the urine is thick. I cannot feel any tumour in the bladder by bimanual examination. If she had a tumour in the bladder, we should probably have seen some blood in the urine after passing the catheter. The presence of a stone in the bladder could also readily be detected on bimanual examination.

The examination of the urine shows it contains both pus and blood; this being so, it is evident that the case is one which cannot be satisfactorily treated in the out-patient department. A careful investigation of all the urine passed will require to be made, in order to decide whether we have to deal with disease of the bladder or of the kidney, and the nature of the disease.

MEETING OF THE SOCIETY OF ANÆSTHETISTS.

HELD ON APRIL 6TH, 1900.

The President, Dr. J. FREDK. W. SILK, in the Chair.

MR. PERCY NOBLE made the following casual communication:—I wish to bring before the Society this evening a somewhat interesting case of paralysis of the respiratory centre in a child during chloroform anæsthesia for the purpose of relieving intra-cranial pressure in basal meningitis; failure to induce respiration until the tension was relieved by operation; recovery from operation; death four days later; with brief post-mortem notes bearing on the case.

A brief summary of the notes of the case is as follows:

Male child, æt. 3½, admitted into hospital on February 23rd, 1900. He was well nourished, conscious, and answered questions; there was, however, marked head retraction, opisthotonos, vomiting, and other signs of meningitis. On February 26th lumbar puncture was performed without anæsthesia; as this did not relieve him it was decided to drain the subarachnoid space, so on the following day I administered chloroform on lint; the operation, which lasted about half an hour, consisted of removing part of the occipital bone and passing a director under the cerebellum until the subarachnoid space was reached and opened, when about six drachms of cerebro-spinal fluid under abnormal pressure escaped; a catgut drain was employed. The child took the anæsthetic very well. On March 3rd chloroform was again administered as the drain was not acting well. The child again took the anæsthetic well and improved for some time, until the drain again failed to act, and it was decided on March 20th to re-examine the wound. I gave chloroform with the child lying almost on its face, with a sandbag under the neck, so that the head was dependent. Before deep anæsthesia was obtained the house surgeon began shaving the skin around the wound, when breathing suddenly ceased. After compressing the chest three or four times respiration again commenced; the shaving and cleansing of skin was being continued, when respiration again ceased.

no additional anæsthetic having in the meanwhile been given. In spite of pulling the tongue forwards, artificial respiration, injection of ether, smacking the face with a wet towel, and such-like remedies, no attempt at spontaneous respiration was made: during this time the heart continued beating regularly (between 80 and 90 to the minute), and fairly strongly, the pulse being easily perceptible; the colour was only very slightly blue, not sufficient to call cyanosed; the pupils small. As we thought the condition of the child was not due to chloroform poisoning, but possibly to some mechanical pressure on the respiratory centre, the operation was performed, and eventually a large quantity of cerebro-spinal fluid escaped under considerable pressure (artificial respiration being maintained all this time); it was not, however, until this fluid escaped that any attempt at spontaneous respiration was made. The child soon recovered consciousness and was put back to bed. Death occurred four days later.

I could not explain this arrested respiration satisfactorily to myself, but thought the dependent position of the head at the operation, after the child had been lying in bed some weeks with the head elevated, might have caused some increased pressure on the respiratory centre in a patient whose ventricles were probably dilated under an increased pressure of cerebro-spinal fluid.

The subsequent post-mortem examination was rather interesting, for, in addition to pus over the base and longitudinal fissure, and dilatation of the lateral ventricles, there was a cyst the size of a thrush's egg in the back of the roof of the dilated third ventricle, which cyst had distinct communication with the third ventricle. It therefore appears to me possible that the dependent position of the child's head at the operation may have caused the cyst to become distended with cerebro-spinal fluid and press on the respiratory centre, and this pressure was not relieved until the wound was opened and the fluid allowed to escape and so to reduce the pressure. I should be glad to hear of any other explanation and criticism of the case.

Dr. PROBYN-WILLIAMS asked Mr. Noble whether the corneal reflex was present when the breathing stopped.

Mr. PERCY NOBLE, in reply, said he believed

the corneal reflex was present just before respiration ceased, because he was asked whether the patient was ready for operating upon, and he replied in the negative, but that the preliminary shaving of the scalp could be proceeded with.

The PRESIDENT asked what quantity of anæsthetic was given; to which Mr. Percy Noble replied that he could not say definitely, as he was giving it from the bottle, which was not marked. He believed it would be about a couple of drachms.

The PRESIDENT said he could not recall a similar case, but he remembered one which he thought bore upon it to some extent, namely, an adult who was operated upon for spinal caries somewhere near the atlas and axis. The patient was about twenty years of age, and he was turned over nearly on to his stomach, and the anæsthetic had to be administered with the anæsthetist practically on his knees underneath the patient. The operation was a long one; it was got through fairly comfortably, but as soon as the patient went back to the wards his breathing became shallow and then ceased. He was informed subsequently by the house surgeon that artificial respiration was kept up for some hours afterwards, during which the patient had occasions upon which he breathed spontaneously. But that did not last, and in one of the attacks he died. Of course the case was not exactly parallel with that of Mr. Noble's, but it bore upon it somewhat, because it was obvious that in Mr. Noble's case the explanation which Mr. Noble gave was the correct one. In his (Dr. Silk's) case he supposed there had been some interference with the fourth ventricle and the respiratory centre in the neighbourhood. It was an important case to bring before the Society, as showing that it was not always the actual measure of chloroform which killed, but very often the disturbance, physical or otherwise, of the respiratory centre, which seemed to have a great effect in these cases.

Mr. EDGAR WILLETT read a paper entitled

"The Administration and Choice of Anæsthetics for Infants and Young Children."

When invited by the Council of the Society to open a discussion at one of the meetings during this session, I had some little difficulty in selecting a subject which had not been previously dis-

cussed. Unfortunately, from the highly special character of our Society, the number of subjects seems unduly limited. I am aware that what I have to say will be found by no means to exhaust the subject, so that I hope others who follow will raise any points that may occur to them, in order to render the discussion more complete and comprehensive.

To begin with a definition is perhaps necessary, and I shall fix the limit of age as being under six years, though these remarks apply more especially to quite young children, by which I mean those under two years of age.

The importance of the subject is, I think, assured, for in looking over my list of private patients I find that in some years these cases amount to nearly 10 per cent.; perhaps this is a large proportion, but it will justify my bringing the subject forward.

When I first attended the late Mr. Mill's classes, he laid it down as a general rule that we should use ether as a routine anæsthetic in preference to chloroform, *except* in patients whose age was under six or over sixty. I think all here will agree that this rule is rather too general; many anæsthetists I know prefer to use chloroform for children and young persons up to twelve or fifteen; it is true that children of six or thereabouts will often take gas and ether quite well through an operation lasting half an hour or more, and I have seen Mr. W. Brain give gas and ether to a child of two for a prolonged operation for the removal of enlarged glands of the neck with complete success, both at the time of operation and subsequently. Again, at the other extreme of life I have given gas and ether to a lady of ninety for the removal of tumour of the breast without causing any embarrassment of the respiration or any undue unpleasant after-effects; no doubt many here have had similar experiences. Still, I think that these cases are rather exceptional, and that chloroform, either alone or in combination, would commend itself to most anæsthetists in the class of patients under discussion. I have heard, for instance, of cases where ether, when given to babies of a few weeks old, has been followed by very severe constitutional symptoms, the children being dangerously ill for a few days with acute bronchial catarrh combined with severe intestinal irritation.

It is because neither pure chloroform nor ether alone seems to me to be the anæsthetic to use for children, *i. e.* that neither gives the best results, either at the time or subsequently, that I have brought the subject forward.

I should like to add to the definition or intended scope of the paper, that I do not propose to discuss the operation for the removal of adenoids of the naso-pharynx, and for two reasons: (1) The limitation of age already given excludes most cases of adenoids; (2) The whole question of the administration of anæsthetics for adenoids, both as regards the choice of the anæsthetic and the position of the patient, has been so thoroughly threshed out quite recently, that further discussion on the subject, at any rate, for the present, seems to be uncalled for.

Granted that chloroform is usually given, the question may well be raised, as it has been frequently asked of me by students, why is chloroform given to children? From the purely theoretical point of view the answer I always give is this: One of the chief dangers from chloroform arises from its action as a poison on the heart, and in this respect it is most dangerous in cases of fatty degeneration of the heart, and what increases the danger is this, that in the absence of other lesions of the heart (I speak under correction and with the greatest deference to those skilled in auscultation) fatty degeneration, when alone present, causes no alteration in the heart sounds, however carefully the heart may have been listened to beforehand, so that its presence cannot be detected. Now in children under six, fatty degeneration of the heart is, so far as I am aware, practically unknown, so that this, the greatest of all dangers from chloroform, is absent; this fact, coupled with the fact that the chest movements of children are, as a rule, free and unrestrained is, I take it, an important factor towards the comparative safety with which, as a rule, they take and recover from the effects of chloroform.

Thus, then, the comparative safety in the case of children as against adults may be given as one answer to the question.

There is a second reason, and that is that children, especially infants, do not take ether well; in this respect my personal experience is at fault, for I have never attempted to give ether

to patients under two, if under four, but I have heard of authentic cases, already alluded to, where the results have not been at all satisfactory.

Thirdly, although it is a small and comparatively insignificant point, the apparatus necessary for ether, even if not accompanied with a large gas bag, has a terrifying effect upon many, if not upon all, young children. All here will agree with me that it is a great advantage to gain the confidence of the patient, more especially in the case of a child.

There is one class of case, and that by no means a small one, where well-marked syncope may occur towards the end of an operation: I mean rickety children. At one time, now some few years ago, a considerable number of children came under my notice for osteotomy for the correction of rickety deformities, and I frequently noticed towards the end of an operation lasting from about thirty to forty-five minutes that the child would rather suddenly become faint, the pupils dilating, and the breathing becoming shallow with well-marked pallor, and this without any increase of shock from the nature of the operation or from any increased dose of the anæsthetic. In all of these cases there was marked beading of the ribs, and the probable explanation seems to be that, owing to the diminished expansibility of the chest walls, the chloroform had after a certain time accumulated in the lungs to an inconvenient, if not to a dangerous amount. I should be glad to hear if other members of the Society have noticed this series of events.

As a routine anæsthetic for young children, pure chloroform does not appear to me to be the best anæsthetic to use, and mainly from the fact that the patients are apt to take a long time to recover from its influence. Nor does ether wholly commend itself, whether given alone, or whether it is preceded by gas, and for reasons already given, *viz.* the unsatisfactory after-effects which not infrequently occur, and the preliminary fright at the sight of the apparatus.

In cases of cleft palate, or for operations on the eye, such as needling a cataract, or the division of the recti for strabismus, when a complete and perfectly quiet anæsthesia is required, undoubtedly chloroform is the best agent at present in use. But there are numerous operations

on young children and babies, among which I may mention circumcision, opening abscesses, the examination of joints, removal of enlarged glands, osteotomies, etc., where a profound and deep anæsthesia is not so essential, and for these I have found that a mixture, freshly made, of equal parts of chloroform and ether, used out of an ordinary drop-bottle on a double fold of lint or Skinner's inhaler, is much better and gives more satisfactory results than does pure chloroform. It is by no means an original plan, and I forget from whom I got the idea. It has an advantage over A.C.E. in causing much less wetting of the lint or inhaler, but its chief advantages are in the character of the anæsthesia produced; perhaps it is rather slower in its effects than pure chloroform, about five or six minutes, instead of three or four, being required for the full effects to occur, when, in a typical case, a most satisfactory anæsthesia ensues. The child as a rule remains of an exceptionally good colour, with rather quickened and perhaps crowing respiration, and with eyes often wide open as if awake; this condition is moreover followed by a rapid return to consciousness, with very little sickness as soon as the anæsthetic is discontinued. No doubt many members are aware of this mixture, but so many practitioners whom I have met have been previously unacquainted with it, and have always subsequently expressed complete satisfaction in its use in the class of cases indicated, that I have ventured to lay stress on it in the way I have done.

There is a question which I should like to bring before the Society, and one on which I should much like to hear the individual opinion of anyone who will be good enough to answer it, giving if possible particulars of individual cases, and it is this, *does shock ever occur during an operation from the fact that the patient is at the time not deeply, or insufficiently, under the influence of the particular anæsthetic?*

Shock in itself is rather a vague term, but I take it that the following will be a fair definition, viz. the presence of several or all of the following symptoms:—Pallor, shallow breathing, a feeble pulse, dilated pupils, sweating. These are unfortunately seen not infrequently in most severe operations, such as amputations through the thigh or at the hip, abdominal sections of all sorts, and

so on, particularly in patients previously run down by pain or prolonged suppuration. But that is not what I mean. Does shock occur during a simple or short operation, such as circumcision, or osteotomy, or an operation for squint, in healthy children, from the fact that the patient is taking an insufficient dose, when it may fairly be supposed that if the anæsthetic had been pushed a little further no shock would have occurred? Personally I do not believe that it does, and I do not remember ever to have seen a case, although I was certainly taught this theory and used to teach it myself.

I am of course aware that this kind of shock like all other kinds, occurs, if at all, in adults as much as in children; but as children are especially liable to shock, and have, perhaps, less power of recovery from its effects than adults, I have raised the question.

Calcium Carbide in Carcinoma of Uterus.—(W. Grusdew, 'Münch. med. Woch.'). Scarcely a day passes that a patient with inoperable carcinoma of the uterus is not brought to Grusdew's clinic. The more extensive his experience, the higher is his appreciation of calcium carbide as a palliative measure in these cases. He places one or two pieces of the carbide, the size of a hazel-nut, on the neoplasm, and packs the vagina with gauze tampons over a cotton wad, protecting the vaginal walls from the chemical action, which consists in the generation of acetylene, etc. The vagina is not packed so tightly as to interfere with micturition or escape of the gas. The patient is then dismissed. In one to three days the vagina is rinsed out, and the application is renewed whenever hæmorrhage or foetid discharges render it necessary. One application transforms the surface of the neoplasm into a clean wound, which bleeds very little if at all. The secretion is diminished and rendered odourless, and there is no danger connected with the treatment if ordinary precautions are observed. Grusdew recommends it as a preliminary measure to cleanse the surface of the carcinoma before cauterising, and he has found it extremely beneficial in curing erosions and benign ulcerations of the vaginal portion of the uterus. In the latter case it is applied in the form of powder.—*Journ. of Amer. Med. Assoc.*, July 7th.

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ORAL SEPSIS AS A CAUSE OF DISEASE, WITH ILLUSTRATIVE CASES.

A POST-GRADUATE LECTURE

Delivered at Charing Cross Hospital, June 28th, 1900,

By WILLIAM HUNTER, M.D., F.R.C.P.

GENTLEMEN,—I desire to-day to call your attention to a subject which, for the last twelve years, in connection with various studies, has greatly interested me,—one I consider of very great importance, one whose importance I think is not sufficiently recognised. I refer to the subject of oral sepsis,—sepsis arising in connection with diseased conditions of the mouth. My attention was first drawn to it in connection with the pathology of anæmia; and since then it has been extended in connection with the pathology of a great number of infective diseases which have one factor in common, namely, septic organisms underlying them. It so happened that about two years ago I had a case which brought to a head my interest in this subject, definitely proving, as it did, the connection between oral sepsis and one of its commonest effects, viz. "septic" gastritis. Since then I have seen a large number of cases illustrating both the frequency and the importance of this condition; illustrating, moreover, what I regard as even more striking, the extraordinary degree to which this condition is overlooked, alike by all parties concerned—the physician, the surgeon, and the patient.

I want to draw your attention to (1) cases illustrating the frequency and importance; (2) the cause of this oral sepsis; (3) the neglect of it; and (4) the effects of treatment.

Case 1.—Let me first relate the case to which I have referred,—one of the most complete I have met with. It was that of a lady who was sent to me suffering from what was thought to be cancer. She had gastric pain, nausea, and sickness extending over some ten months. She presented the typical sallow, wasted, cachectic

appearance. On examination I could find no trace of cancer, either in her abdomen or in her pelvic organs. On inquiry I found in addition to this gastric pain, which she suffered from intermittently and required opium to relieve, that there was a constant bitter taste in the mouth and inability to taste her food. Her nausea and sickness had no relation to food, coming on usually in the morning. On examining her mouth what struck me was the cleanliness of it. Her teeth, with the exception of four, were absent. She had two teeth plates in, one above and one below, scrupulously clean. Her gums also were very clean. The four teeth presented at first sight nothing very striking, except that they were black. On further examination I found that three of these teeth were loose, and on squeezing them pus oozed out all round them. The question which occurred to me was, could this condition of sickness and vomiting be caused by constant swallowing of this pus? The provisional diagnosis made was that it was due to this continual swallowing of pus; and I directed her, as the first part of treatment, to have the stumps removed at once, preparatory to any further treatment. This she at first demurred to; she said the stumps were very old friends of hers, and she had had them like that over twelve months. Her illness had dated from eleven months ago. However, she went and had them removed next day, and the result was that in a week's time she reported herself as having had only one return of sickness, namely, on the day after the extraction of the stumps. She had lost the bad taste in her mouth, and she was able to taste food properly for the first time for months. This improvement did not last, for three days later pain came on, with sickness and vomiting; and this continued, on and off, for two or three weeks. At the end of three weeks I got a specimen of her vomit and examined it microscopically; it consisted of inflammatory exudation, namely, blood, fibrin, leucocytes, and gastric-cells; and in the midst of them were masses of pus organisms, streptococci, and staphylococci. This, you will have noticed, was three weeks after removal of the source of infection from the mouth. The diagnosis of "septic" gastritis was thus, I consider, confirmed, and I proceeded to treat her accordingly. Up to that

time the patient had been struggling to get about, but she was now really ill, and had to take to her bed with this gastric pain. Her temperature was 99°. The treatment which I applied was the following:—I fed her entirely on peptonised milk and gruel, beginning with one and a half pints daily. I applied counter-irritation to the stomach, and gave sedatives. With a view to combating this streptococcal infection, I gave her salicylic acid, three-grain doses three times a day. Improvement was immediate and continuous, and sickness and pain were entirely checked from the first twenty-four hours. The tongue lost its raw, angry look, and became normal. The pulse fell from 102 to 70; temperature, which had been slightly raised, fell to the normal. In ten days' time she was able to go out driving, and to return to her home in the West of England. I kept her on peptonised milk gruel for another month, at the end of which time she reported herself quite well, and that she had had no return of the pain. A month later she reported herself as still free from pain, and gaining in weight. In three months' time she had gained a stone in weight on her milk diet. Fifteen months later she also reported herself well, and that she had had no return of the sickness or pain. The salicylic acid was continued from two to three months.

When I met with this case, I studied it as an individual case interesting to myself; that is to say, I put to myself the question whether the swallowing of pus could cause such extraordinary untoward effects. When I found that it did do so, I looked to see whether this condition was recognised, as it seemed, even at first sight, a very obvious and possible source of trouble to the stomach. I found a very interesting condition of matters, namely, that while there was much mention of errors in food and drink, habits of eating, insufficient mastication (from absence of teeth or pain in the teeth), as causes of gastric trouble, in the literature of the subject there was no mention of gastritis of any kind or degree being caused by *infection from the mouth*.

I have described in some detail this case, because it is complete to an exceptional degree in this relation, viz. in the actual proof of the infection of the mucosa of the stomach by pus organisms. *Three weeks after the removal of all*

source of trouble from the mouth, the exudation which came from the stomach wall was a purely catarrhal inflammatory exudation, filled with these streptococcal organisms. It is also interesting in the fact that the condition was cured by direct antiseptic treatment of the stomach with salicylic acid.

Up to the time I met Case 1, my suspicions with regard to the teeth were only suspicions. I knew how infective the organisms of dental conditions were, but I had no proof that infection from this would actually cause disease. This case which I have related was particularly striking, because I made no change as regards her powers of mastication. The only change made was the removal of three useless stumps. These she regarded as old friends, and was averse to having removed,—a point to which I draw attention, as it is one you will find illustrated every day in your practice in these troubles.

The case demonstrates, to my mind, a relationship between dental cario-necrosis and gastric catarrh of a closer and more direct character than any hitherto recognised. This relationship may be briefly described as follows:—There is a limit to the capacity of the stomach to resist indefinitely, for long periods, the presence of pus organisms. The power of the stomach to destroy such organisms is due solely to the presence of free hydrochloric acid. This power becomes progressively weakened when, through any cause, you have an increased and continuous supply of pus organisms, with diminished secretion of hydrochloric acid. These conditions are precisely those produced in chronic catarrhal conditions of the stomach; and in time this catarrh of the stomach, which we have all looked upon as an irritant catarrh—a result of fermentation,—becomes *septic* in its nature, and is really due to actual infection of the stomach by septic micro-organisms.

The liability to this infection must naturally be greater if the organisms introduced be really disease-producing organisms,—that is to say, those able to set up inflammation and suppuration elsewhere. That is precisely the character of the organisms found in connection with these conditions of mouth and teeth. Dental cario-necrosis has for a long time been regarded as a disease by itself, due to action of some acid on

the teeth. But recent observations, especially of Professor Miller, and Professor Arkovy and others, have shown, and proved to demonstration, the truly bacteriological nature of this dental carious process. Moreover with every condition of dental necrosis you have the presence of pus organisms, both streptococci and staphylococci.

Now the significance attaching to what I have just said is not the mere presence of these organisms in the mouth. Disease in the mouth or stomach is not the result only of the presence of organisms, but a question of the actual *amount of resistance* on the part of the tissue, and the *dose or virulence* of the organisms. In the mouth the question of dose is a most important one. It is important when there is one necrosed tooth; but when there is a whole mouthful of such teeth, lying in foul, suppurating sockets, surrounded by inflamed, suppurating gums, the infective power is very great. The effect is two-fold—first of all, locally; and secondly, on the body generally. The local effects are in the mouth itself, and are evidenced by the oral sepsis so common, namely, inflammatory stomatitis and gingivitis of every degree of intensity, erythematous, pustular, ulcerative, or even gangrenous. It is evidenced also by the presence of alveolar abscesses in connection with the teeth, by the presence sometimes of gumboils and, in rarer cases, by the presence of suppuration in connection with the maxillary sinuses and in connection with the ethmoidal cells. All these local conditions are illustrated by the cases I have to describe. But the effects are not limited to these, because if you have a continual source of infective generation going on round these teeth, you may have infection occurring lower down, especially within the stomach.

The number of organisms which enter the stomach from the mouth is very large. Most of them are destroyed by the gastric juice, but that is by no means true of all. As many as eight out of twenty-five different kinds of organisms are, according to Professor Miller, to be found in the stomach contents; and the observations of Professor Macfayden and others show that during digestion the gastric juice is only sufficiently potent to kill organisms during active digestion—not in the intervals between digestion.

When the acidity reaches a low level, a large number of organisms may be quite capable of living in the gastric juice. In long-standing dental disease the conditions are precisely those most likely to lead to infection of the stomach, namely, on the one hand there is diminished resistance—that is to say, lessened gastric acidity; on the other hand there is an increased dose, due to the continual swallowing of these organisms. That under such conditions fermentation goes on we have already recognised; but the case (Case 1) I have related shows that the effect of these organisms is not limited to the fermentation of food products, but that really in time the presence of these organisms may lead to infection of the mucosa, and produce what I would term a "septic"—as distinguished from a simple irritant—gastric catarrh. These considerations as to the effects, local and general, of long-continued dental and oral sepsis, are of no mere pathological interest; they are, to my mind, of the supremest practical importance; but they are recognised to a degree which is altogether insufficient. In almost daily practice one meets with gastric catarrh associated with dental decay. There is an ashy-grey look, and a general languor about the patient, which I regard as the result of actual long-continued septic absorption, especially when associated with the local symptoms of distaste for food, periodic nausea, and so on.

These considerations, as I have put them before you, may appear so obvious that they require no further emphasis. But the cases I have to describe illustrate two points—first, the frequency of the conditions; and secondly, the extraordinary way in which the most remarkable conditions of early sepsis are overlooked, while the patient is being all the time sedulously treated for the local effects.

Case 2.—A few weeks after I published the first case, I was asked to see a lady who for several years had suffered from remarkable periodic attacks of fever and rashes, and who had marked nervous disturbance. These attacks had come on at irregular intervals for two or three years, and I was called to see her when she had one of her rashes. I found she had a typical blotchy septic rash over the legs, arms, and body. Her history was that about a month or two previously,

her dental surgeon, having had his attention drawn to it by the case I have narrated to you, had insisted on removing a tooth-plate from her upper jaw, *which had partly grown into the upper jaw*, and had been there for several years. Her condition was one of profound sepsis. Her periodic rashes, gastritis, and nervous disturbances were the acute manifestations of that.

Case 3.—Shortly afterwards I saw an old gentleman, a man of strong build and fine physique. He came complaining of sickness and nausea, with disturbance of digestion, and a foul taste in his mouth. He could not eat butcher's meat as it tasted so bad. These symptoms had lasted twelve months when I saw him. On examination in the usual way, I found that his tongue was red and raw, looking like a piece of raw meat; both upper and lower gums were angry, and red and inflamed. He had two plates, one in the upper jaw, from which all the teeth had been extracted, and one in the lower jaw. The latter was removed with difficulty; it had not been taken out for a month or more, and had become fixed. There was an extraordinary amount of decomposing septic material around the plates and beneath them. The lower jaw contained three black teeth, one of them loose, in addition to four old rotten stumps, one of which was loose. The diagnosis I made was subacute septic gastritis. The treatment was to boil his plates, to go at once to his dentist to show him the condition of his mouth before any treatment was commenced, so that the dentist might recognise that the trouble had been caused by these rotten teeth. The patient was then put upon milk diet. A week later he returned and said his dentist had seen nothing to remove. Yet even now one rotten stump was so loose that it could have been removed with one finger. He had been twice sick since I saw him, and had brought up a lot of black offensive matter. Since bringing that up he was better, and took his food better than for many months. "Yesterday he took a cutlet, which he enjoyed." The mouth was now clean, but was still a little red. The tongue had lost all its original beefy look. The gums were better, but sore, and there was still some stomatitis. I ordered him to scrub his mouth night and morning with disinfecting powder, and to

paint the gums with an astringent wash, and to try another dentist.

Case 4.—Shortly after this, I had the case of a lady who was brought to me for the following symptoms:—For fifteen to twenty years she had suffered periodically from the most intense salivation at intervals of five or six weeks. It made her so ill that she was obliged to take to her bed. The attack usually passed off after what the doctor called a “diarrhœa attack.” On examination I found that she had the most extreme stomatitis all over her mouth; an acute, inflammatory condition, with psutules radiating up from carious roots and fangs. She had two plates, one above and one below, both of which she said were ill-fitting, so as to cause her discomfort. *She had had those plates for fifteen to twenty years, unchanged, and during that time she had only cleaned them with her tooth brush.* I ask you to imagine the condition of sepsis in connection with that.

Gentlemen, these cases can be multiplied indefinitely. In every out-patient ward in every hospital you can see them by the dozen. The condition is so marked that you have only to look into the mouth of such patients to see what is the trouble. As Professor Miller says, “A glance into the mouth reveals such a condition, that it is astonishing how it could ever be overlooked.” But it is not simply conditions of stomatitis which are produced in this way; sometimes the results are much worse.

Case 5.—A short time ago I was asked to see a lady for profound septic poisoning. She had a temperature of 105° and 106°, and a most extensive condition of ulcerative and almost gangrenous stomatitis. She had been in this condition for seven to ten days. She had had a tooth removed, and the root still remained. There was an abscess in the maxilla, and she had a sinus, and about it pus lay around the gums. She had a sloughing condition of her hard palate. The treatment employed was local antiseptics, scrubbing the parts with one in twenty carbolic acid lotion; and by that means, in forty-eight hours, despite that woman's condition, the whole condition of gums and mouth looked fairly normal, though at that time she was almost moribund with septic pneumonia. I draw attention to this case to show how resistant

the mouth is, and how—if it is properly cleaned—the infection can be destroyed. It illustrates very well the recuperative power possessed by the mouth.

Case 6.—Another case was that of a youth who had inflammation of his gums set up by withdrawing his tooth. Extensive stomatitis set in, and spread from point to point until the teeth became loose and necrotic. Half the lower part of the jaw became completely necrosed, and there was a foul, gangrenous condition of the whole of the superior maxilla, an acute and profound septicæmia, hæmorrhagic nephritis, and death. I will show you the specimen. This case still further illustrates to you the severity of the effects in connection with pathogenic microorganisms.

Case 7.—I have now to point out that in connection with this dental caries you may have these pyogenic effects latent. I show you the jaw of a man who presented no dental history during life, so far as could be ascertained. He died of pernicious anæmia. Post mortem, the condition found was the one you see here. The teeth were necrosed in their sockets, which presented a sodden appearance, and in this particular case you will notice that at the bottom of one of them is an alveolar abscess the size of a small hazel nut, leading by a sinus to the necrosed tooth. In connection with another tooth you will see there is a smaller pus centre. In this case there was suppuration in the ethmoidal sinus on the left side.

Summary.—With regard to *treatment*, what I wish to bring home to you is that it is not the stomatitis, or the dental caries, or the absence of teeth, or any disturbance of nutrition in connection with these stumps that causes all these effects. The condition in one and all is that of sepsis—I mean what is understood in surgery by sepsis,—that is to say, you have to deal with pus-forming organisms which are constantly present in the mouth in connection with necrosed teeth.

Now I want to point out to you the inconsistency of us all. No physician would tolerate for a moment that a person who has a foul septic ulcer on his arm should periodically suck it. A surgeon may be said to spend his life in combating septic infection; he surrounds himself at his

work with everything scrupulously clean; he goes to every expense, and initiates arrangements of the most perfect order in his operating theatres; he will not even touch the skin without scrubbing or doing his best to get rid of all possible infection. But while doing all this he will, without the slightest regard, operate on his patient when the mouth is in a septic condition, full of necrosed teeth, and full of the effects of necrosis. Then comes the dental surgeon, who does so much in these days of conservative dentistry to relieve the conditions in the mouth; he also will not scruple to supply, every now and then, tooth plates which will not fit; cap a diseased tooth with a most beautiful gold cap; and he will do worse, viz. beautify two carious teeth with gold caps and in order to make the operation complete, he will extend the gold across the intervening space, resting only on the gum below. The result is a pocket formed under the bridge which is a perfect culture-bed for organisms which cannot be got at.

Case 8.—A case where salivation, with gastric symptoms, was produced in a patient by a bridge of this kind, which excited a local stomatitis. The bridge was removed and found to be filled underneath with pathogenic organisms. The salivation ceased with the removal of the bridge.

Case 9.—The same patient had a gold cap, and that was removed at the same time as the bridge. To complete the operation the dentist put another cap over the tooth. Some weeks later there was a recurrence of the salivation, with the symptoms of local stomatitis, or rather, gingivitis around the capped tooth. On removal of the gold cap, it was found that the gold cap covered a minute carious cavity in the neck of the tooth close to the edge of the gum.

With regard to the treatment of these cases, what we want recognised on the part of all physicians, surgeons, dental surgeons, and patients, is the septic nature of this condition of caries of the mouth. The gastric trouble is not the result of any dyspeptic trouble, or of ill health, or of insufficient mastication; but is the result of sepsis caused by the carious teeth.

The matter, however, is important not only from the point of view of the gastric trouble, but of the infections in the body generally caused

by pathogenic organisms *locally*, acute and chronic tonsillitis, pharyngitis, otitis, follicular abscesses, glandular swellings in the neck in connection with diseased teeth; or more *remotely*, as ulcerative endocarditis, meningitis, obscure septicæmia complicated by purpuric hæmorrhages, pyæmia, osteomyelitis, in fact, the whole series of conditions caused by pus organisms. The chief problem with regard to these conditions is to find out where the pus organisms have gained entrance. They are not ubiquitous, but are definite organisms causing pus formations. We take most elaborate precautions to ensure ourselves against typhoid infection, either from drains or from water, and we take great precautions to protect ourselves from tubercle; and there is no reason why, when we are doing all this, we should allow the most accessible part of the body to remain a favourable seat not only for the propagation, but for the actual production of them. Therefore I consider that in regard to oral sepsis there is a wide field open for preventive medicine by the practice of oral antisepsis. When I say oral antisepsis, I do not mean any general application of mild astringents or antiseptic washes. I mean (1) the direct treatment of each lesion in connection with a diseased tooth by strong antiseptic solutions: carbolic acid (1 in 20 or 1 in 40) to be applied by means of a camel's-hair brush or a piece of cotton-wool directly over the diseased root. This application should be applied to each diseased tooth as long as the patient delays having the tooth removed. A teaspoonful of 1 in 20 carbolic acid in half a tumbler of water forms an agreeable mouth wash. (2) Still better, it can be done by removing all diseased stumps and roots, in particular those lying underneath any tooth plate. (3) There is a necessity for recognition on the part of the dental surgeon that the conditions he deals with are in all cases septic; he must not be simply content to give his patient a tooth plate and say "Wear that." The patient will have to be educated, and shown that these plates are the cause of septic trouble unless they are actually boiled. (4) There must be an entire avoidance of any dental apparatus which cannot be removed, and therefore which cannot be kept aseptic. This is a field of preventive medicine which I think you will find can be worked in with

the most extraordinary success by the doctor, the surgeon, the dental surgeon, and the patient.

There is another matter of very great practical importance. Who is to do all this? The physician sees the mouth condition, and sends the patient to the dentist. The chances are at least ten to one that the patient will not go there, because the dentist is associated primarily in his mind with the extraction of teeth. The surgeon looks upon sepsis in the mouth as coming within the domain of the physician, unless there be an actual disease of the jaw. The dental surgeon will treat the diseased tooth dentally, but he will not have his patients come back in order to be treated locally. So the poor patient is in the position of being driven about from one to another. I have been impressed by the neglect of the patient in this way, and I have tried, as I have narrated, the effect of sending patients in an extreme condition like that I have described (Case 3) to the dentist. He came back without having anything done. Therefore the question is for each one to recognise that it is not an affair of the other. If you see a follicular tonsillitis or a quinsy you do not immediately pass the patient on to a throat specialist; you treat it yourself. This condition of oral sepsis is one which can be treated successfully by all, even by the patient himself, provided its septic nature and its importance as a disease factor be fully grasped.

The effects I have described are very common. That they are not even more common is solely due to the remarkably resistive powers possessed by the mucosa of the mouth. How rapidly wounds in the mouth heal is well known. Now an important point in the cases of which I am speaking is, that pus derived from soft tissue is not so virulent as that which is derived from bone (tooth). It is only now and again, when the bone condition is very bad and the resistive power of the mucous membrane of the mouth is very much lowered, that you get the bad conditions which I told you of in the early part of the lecture.

The great resisting power of the mouth is, however, no reason why such conditions of oral sepsis should be overlooked. How would you regard a physician or surgeon who allowed a patient to go about for many months, not to say years, with several small follicular abscesses in

his tonsils. You would think it very neglectful; and if that patient came to you with a sallow look and you saw pus on the tonsils, you would say at once, "Here is the cause of the condition;" and rightly so. But it is the rule to neglect similar cases in connection with the teeth. It will show you how much it is overlooked when I tell you that notwithstanding all the literature pointing out the troubles due to septic organisms in the tonsils, oral sepsis does not seem to have received notice as a cause. Even in connection with stomatitis, mention is made of the irritation caused by a pointed tooth, or the stem of a pipe used by the patient, but nowhere is the reader told to look out for septic inflammation around the tooth. In connection with gastritis, mention will be found of the want of teeth, and the consequent imperfect mastication, as causes of indigestion; but nowhere, not even in the latest treatises on diseases of the stomach, will mention be found of necrosed teeth as a source of infection. Most strange of all, even when the condition of the stomach is one of suppurating gastritis, the rôle of every conceivable factor (errors in food and drink, etc.) receives fullest consideration; but not once is mention made of oral sepsis, even as a possible factor, although in one of the most striking cases related the condition described as preceding the gastritis was that of septic ulceration all round the mouth, with necrosis of the jaw, and splinters of bone lying about.

WE have recently received for notice from Otaduy & Co., of 20, High Holborn, samples of the Rioja Vintage Wines. They are robust, full-flavoured wines, with tonic properties of the highest order. The almost entire absence of sugar and their remarkable freedom from acidity mark them out for use in rheumatic and gouty cases. In anæmic conditions their value is obvious and undoubted, especially as they are free from that objectionable "*gout de terre*," so noticeable in the host of cheap French wines now in vogue. A further guarantee of excellence is furnished by the fact that these wines are under "The British Analytical Control," every bottle sold bearing this important organisation's stamp of purity.

ACUTE APPENDICITIS WITH WIDE-SPREAD PERITONITIS.

A CLINICAL LECTURE

Delivered at the General Hospital, Birmingham, June 7th,
1900,

By GILBERT BARLING, B.S., F.R.C.S.,
Surgeon to the Hospital.

GENTLEMEN,—I want to bring before you to-day the case of a patient upon whom I operated four days ago for acute appendicitis, with peritonitis spreading widely from the right iliac fossa. Within a few weeks I have operated upon two private patients with almost exactly similar conditions, and the three cases together form an extremely interesting group. I therefore propose to give you the history of the three patients in some detail, and comment upon them, believing that in this way some useful information may be conveyed.

Case 1.—Male, æt. 25, was admitted to the hospital, under the care of Dr. Saundby, with a history of two days' illness. The patient was at work on May 31st, and that evening was seized with pain in his bowels. He went to work, however, the next morning, but had to desist, owing to vomiting and pain, chiefly on the right side of the abdomen and around the umbilicus. His bowels did not act, but there was no history of past constipation. Some degree of sore throat was complained of on May 29th.

On admission (June 2nd) the patient's pulse was 96, his temperature 101.2° , subsequently rising to 102° in the evening. His face was indicative of pain; there was tenderness over the right iliac region, but, owing to muscular rigidity, it was impossible to say whether a tumour existed or not; vomiting was frequent. Hot fomentations were applied to the abdomen, and nutriment was restricted to small quantities of milk and barley-water by the mouth.

I was asked to see the patient on the morning of June 3rd, and learnt that he had passed a bad night—restless and painful. His pulse was 80, his temperature 98° ; his face looked worn and tired, but not pinched; the abdomen was very rigid and tender over the lower half, and there was diminished resonance above the pubes, whilst vomiting was persistent. It was so difficult to reconcile his bad

symptoms with the fall in pulse and temperature to normal, that I expressed the wish to see the patient again in a few hours, suggesting meanwhile that nothing should be given by the mouth, but that an enema should be administered to empty the bowel, and nutrient enemata afterwards.

On seeing the patient about eight hours afterwards I found the vomiting still persisting, and the other conditions much the same, except that the pulse had quickened to about 90. Operation was therefore resorted to, and through the usual oblique incision across the omphalo-spinous line the vermiform appendix was removed. It ran up behind the ascending colon for about three inches, and was intensely inflamed and necrotic, but was not perforated, and did not contain a concretion. Its distal extremity was dilated to the size of the end of one's thumb, and burst during manipulations to remove it, liberating a drachm or so of pus; but there was no actual stenosis of the appendix, the obstruction to the escape of the pus it contained into the cæcum being due merely to the swollen mucous membrane. A quantity of pus lay around the cæcum, and extended down into the pelvis, there being no limiting adhesions in any direction. The abdomen was therefore opened in the middle line, about a pint of pus evacuated, and through the two incisions the lower part of the abdomen was carefully cleaned of pus by sponging. A drain was placed in the right iliac fossa and one in the pelvis, both incisions being then carefully sutured in layers with silk-worm gut.

Since the operation there has not been a minute of anxiety about the patient, and recovery is practically assured. (*Note.*—The patient was allowed to be out of bed on a couch June 21st, and was discharged well on June 28th.)

Case 2.—I was called in consultation in March last to see a gentleman, æt. 27, whose history was as follows:—On March 19th he was seized with central abdominal pain and vomiting, and was put to bed. The pain and sickness existed in various degrees for the next three days; the right iliac fossa was tender, and the pulse, at first 120, dropped to and below 100.

On March 23rd the patient was so much better that he wished to get up, but was not allowed to do so. This day, as the bowels were constipated, a dose of liquorice powder was administered, which caused several actions; and at 10 p.m., when at

stool, the patient was seized with intense pain in the lower half of the belly, especially in the left iliac fossa. I saw him at 3 a.m., and found him crying out with pain, although he had received morphia hypodermically until the pupils were well contracted. The lower half of the abdomen was extremely rigid and tender, especially in the left iliac region, and there was diminished resonance over practically the whole of this region.

The pulse was 120, and quite soft; the temperature was 99°.

Operation was performed at 5 a.m. on exactly the same lines as in Case 1. The appendix was found hanging down into the pelvis, protected to some extent by adhesions, but gangrenous and perforated near its distal end. Through the two incisions a large quantity of pus was removed by sponging, and both incisions were drained. The first few days were anxious ones. The patient was somewhat jaundiced; he hiccupped at times, and was delirious at night; but at the end of a week all bad symptoms had disappeared, and by the sixth week after operation the patient was well.

Case 3.—In April of this year I was asked to see a married lady of between thirty and forty years of age, whose history was as follows:—For years she had been troubled with constipation and with stomach indigestion, gastric ulcer being for a time suspected. On April 1st she suffered from pain in the epigastrium, such as she had frequently experienced before; and on April 2nd, as the pain in the epigastrium was still bad, the patient stayed in bed and took a light diet. Her temperature and pulse were normal in the morning and evening of this day; the bowels acted with an enema. On April 3rd, at 7 a.m., the patient shivered and vomited; the temperature shortly afterwards was 99·8°, the pulse 90. From this time on vomiting took place frequently until evening, when it practically ceased, and the pulse and temperature steadily rose.

Abdominal pain was severe, and was now felt in right iliac fossa and in the pelvic region, and no longer in the epigastrium. For its relief three hypodermic injections of morphia were given in the course of fourteen hours. Flatus was passed spontaneously, and pain followed micturition.

When I saw the patient at 10 p.m. the pulse was 132, quite soft; the temperature 102°; her expression was fairly good. She complained scarcely at

all of pain, but described herself as feeling much better, and as desirous of being allowed to go off to sleep. The lower half of the abdomen was very rigid, but tender only on firm pressure, which produced a desire to micturate. There was diminished resonance in the right iliac fossa.

Operation was at once performed on the same lines as in the two preceding cases. The vermiform appendix was found firmly adherent in the ileo-cæcal pouch. Its terminal portion was ulcerated off, and a fæcal concretion lay beside it, whilst another concretion existed in the proximal portion. A quantity of stinking pus, estimated at something over half a pint, lay around the appendix, and the pelvis and adjacent part of the abdomen was occupied by a large quantity of pus, foetid, but not blood-coloured. As in the other cases, a second and median incision was made, and through the two incisions the lower part of the abdomen was carefully cleaned by sponging, and then drains were employed.

The patient's condition was one of anxiety on the day following the operation, when black fluid—altered blood—was vomited; but after this a steady but slow convalescence followed. As is frequently the case in similar conditions, the sutures in both incisions soon cut out, the wounds gaping considerably; but at the end of seven weeks they were entirely and firmly healed.

It is, of course, a matter of great satisfaction that these patients, all suffering from such severe illness, should all make such a good recovery. This alone would not justify me in bringing them to your notice, but really valuable lessons are to be learnt from them.

In the first instance, let me impress upon you that in *nearly* all cases of severe appendicitis there is a time at which successful operative interference is possible; but this time may be very soon after the initial symptoms, or it may arise suddenly after a few days of well-doing. When operation is to be done must be determined not by the particular day of the disease, but by the urgency of the symptoms, and by a general survey of the patient's condition. Case 2 was doing well, when an accident occurred—possibly the rupture of a small and well-localised collection of pus into the peritoneal cavity, or it may be the actual perforation then happened—with resulting intense peritonitis, calling urgently for immediate operation. In Case 3 it is difficult to

say when the appendicular inflammation commenced; but the patient was under careful observation, and I incline to think it was not more than sixteen hours before I saw her. Yet at my visit there was widespread suppurative peritonitis, calling urgently for operation if a calamity were to be prevented. The rapidity with which a large purulent peritonitis may arise is remarkable. I have seen pus which could be measured almost by pints six hours after a gastric perforation.

Whilst the lesion in these cases was very grave, it must not be overstated. It would be incorrect to speak of it as general peritonitis, though widespread it assuredly was; yet I have little doubt the mischief was in each case limited by the transverse colon and its mesentery. These structures form a very valuable mechanical barrier, protecting the upper part of the belly cavity. Below this the infection easily involves the small intestine and its mesentery, there being no mechanical obstacle. Once infection extends to parts above this barrier, especially to the subphrenic regions, then I believe the art of the surgeon is of little avail.

If you will recall the prominent features of the three cases I have related, you will observe that in each case there was one or more pronounced *bad* symptoms. In Case 1 it was persistent vomiting, when in all other respects the patient appeared to be doing fairly well. In Case 2 it was the intensity of the pain which dwarfed all the other features of the case; and in Case 3 it was the increasing rapidity of the pulse, which rose from 90, after the rigor at 7 a.m., to 132 sixteen hours later, increasing steadily from hour to hour. The patient at this time was well under the influence of morphia, and felt so much better that it was a matter of surprise to her when operation was insisted upon immediately. This is the danger of morphia: it is apt to mislead patient and doctor, and perhaps even more the patient's friends. I do not say to you never give morphia—in appendicitis it is often necessary to give it for the relief of intense pain,—but I would urge upon you *not to go on giving it*. *Persisting intense pain* should not be treated by repeated doses of morphia, but by operation.

Case 2 illustrates another important point—the possibility of doing harm by administering purgatives. When the disease formerly called typhilitis, now appendicitis, was believed to be due to faecal impaction in the caecum, treatment by purging was

a reasonable outcome of erroneous pathological ideas. The operating theatre and the post-mortem room have shown us things as they really are—not faecal impaction, but an inflamed vermiform appendix, which nature is doing its best, in the majority of cases, to circumscribe by adhesion. Do not let us ignore this. If it is necessary to empty the bowel, let it be done in the gentlest manner possible, and with the least excitement of peristalsis. This will generally be attained with the use of simple enemata.

I cannot make a better conclusion to my remarks than by repeating what I have made almost into an aphorism:—"The great point in these cases is not to regard any one condition as essential to diagnosis. It is desirable to dwell not so much upon the *absence* of any particular feature, as upon the *intensity* of those that are present."

Concerning Joint Effusions in Hæmophilia.

—By Dr. I. R. Manteufel. In speaking of the treatment of these cases the writer says that as the cause of hæmophilia is as yet unknown, there is no specific remedy for this diathesis. Z. Manteufel used Zymoplasm. In the case of a boy who had a hæmorrhage from the gum after the extraction of a tooth, after all other measures had failed, Manteufel applied Zymoplasm to the cavity on some absorbent cotton, with the effect of gradually arresting the bleeding. Certain orthopaedic appliances can be used in hæmorrhagic effusions in the joints. The joints must be protected from injury by splints. In the local treatment such measures as rest, the application of compresses, bandages, massage, and active or passive motion may be useful. If the pain is not too severe the patient may be allowed to walk about. A moderate amount of motion is, in fact, beneficial. The ether spray may be used to allay the pain, but it must not be applied unless the pain is very severe, for it inhibits absorption of the effusion. In the advanced cases in which suppuration occurs in the affected joint, the prognosis is very bad, as very little can be done to reduce the deformity or to restore motion once the ankylosis is firmly established.—VRATCH., *N. Y. Med. Journ.*, Aug. 25th, 1900.

A DISCUSSION

ON THE

ADMINISTRATION AND CHOICE OF ANÆSTHETICS FOR INFANTS AND YOUNG CHILDREN

At a Meeting of the Society of Anæsthetists.

THE PRESIDENT said Mr. Willett's paper * was a very valuable one and had raised many points of great interest, and perhaps he might say points upon which there was reason to believe there was difference of opinion.

Mr. CROUCH said he had listened to Mr. Willett's paper with a considerable sense of disappointment, because he thought Mr. Willett had failed to lay stress upon one point, namely, the treatment of the child before the anæsthetic was given. He did not mean that Mr. Willett ought to have given the members a detailed account of the conversation he had with the child, but as he mentioned that the bag and apparatus were terrifying, he might have made some suggestions as to how to give the anæsthetic without the production of such apparatus. The children to whom anæsthetics were given might be divided into two classes:—in the first class was the child who knew nothing about the anæsthetic at all; and to the second class belonged the child whose parents had rehearsed the affair, telling it to breathe quickly and that it would then go to sleep, and so on. Of the two classes, the latter was by far the more difficult to deal with. He found that if the apparatus was not brought forward for the child to notice, a great deal was done towards calming and soothing it. For children who were old enough to be frightened at the anæsthetic he had made a hollow finger-ring which was perforated underneath, behind which was a tube connected with his Junker. He then talked to the child with his hand near its mouth, with the anæsthetic running continuously, and in that way the child became unconscious without really noticing it. He then put on the mask, and the child was put and kept under sufficiently for the operation. Surgeons, in the cases where he had employed it, regarded it as a toy, and failed to appreciate it, but in practice he found that the parents did appreciate it, and,

especially in the case of young children, they had to consider the parents.

Another point raised by Mr. Willett was as to whether shock was helped or caused by insufficient anæsthesia. He gleaned that Mr. Willett was a little sceptical about shock occurring, even in adults, from that cause. He (Mr. Crouch) had lately given a series of anæsthetisations to young children, abdominal cases, all of them under three years of age. In four the disease was malignant adenoma of the kidney, one a malignant ovary, and the other sarcoma of the intestine. The first three, who were kidney cases, all died; they had enormous shock at the time, and he was certain it was because he funk'd giving them enough chloroform. The fourth case was that of malignant kidney, at the Evelina Hospital, and in that case he gave a sufficient quantity of chloroform, as he thought, but the moment traction was made on the kidney the child stopped breathing. He then pushed the chloroform further, and the kidney was rapidly removed; the child had absolutely no shock, and the next day wanted to sit up and have its meals. In both the remaining cases he followed the same procedure of deep narcosis, and there was no shock; indeed, the patients did not even have a subnormal temperature. In adults, in whom he presumed the cause of shock was the same, he had at first, at the Samaritan Hospital, a very great deal of trouble on account of the amount of shock which was caused. Now he was bolder and gave more chloroform, and the surgeons remarked that the patients did not have the same amount of shock that they did previously. He had seen so many of those cases that he did not doubt that there was benefit from giving a very much deeper anæsthesia than he had been in the habit of giving before.

He was interested in Mr. Willett's remarks as to why he gave chloroform to children in preference to ether, because he (Mr. Crouch) had met with the same difficulty. Students asked why chloroform was given to children instead of ether, and he had not been able to give them a likely theory, but it was a great comfort to hear of a reason being given which would apparently hold water. Still, in adults, if he suspected fatty degeneration of the heart he was not sure that he would give ether in preference to chloroform,

* See p. 317, 'Clin. Journ.,' Sept. 5th, 1900.

for he had lately given very much chloroform for all classes of cases, and he was beginning to think it was really productive of less strain on the heart.

Mr. C. G. BURTON said he had been very much interested, as resident Medical Officer at a Children's Hospital, in listening to Mr. Willett's paper. At the North-Eastern Children's Hospital, where he had given anæsthetics 2000 times, they had used in many cases A.C.E. mixture, and they had found in children over two years of age that the effects were better than they were from chloroform. He had not previously heard of the mixture of equal parts of chloroform and ether which was mentioned by Mr. Willett, but he would try it, because it was a great disadvantage to have to use so much anæsthetic for a long operation. Again, if used with a mask there was so much moisture, which was inclined to get on the face and cause marks. With regard to shock, he had never seen a case of shock after operation which he could attribute to insufficient anæsthesia. If the patient felt pain it was more likely to rouse him up than to produce profound shock. In children he hardly believed in shock occurring through pain alone. In certain cases he had seen somewhat severe operations commenced on children without any anæsthetic. Children seemed able to stand any amount of pain and it did not seem to do them much harm, whereas in adults it appeared to have serious effects.

Mr. WHITE said Mr. Willett's paper was extremely interesting, and it must be particularly so to those advocates of chloroform who were present. Personally, he might say that in the early part of Mr. Willett's paper he disagreed with everything he said. He did not think a limit of age could be laid down with regard to whether ether or chloroform could be given. He had given ether to all cases on which he could fit the mask, and he could not say that he had found chloroform safer than ether. It was necessary to refer back to the statistics showing the relative deaths from chloroform and ether, when it would be found that 10 per cent. of the chloroform deaths occurred in children. As far as the after-effects of anæsthetics in children were concerned, he did not think that there was more sickness after ether than after chloroform. He certainly agreed with Mr. Willett that chloroform must be

the anæsthetic for cleft palate and cases of that sort, but that had to be given because the ether apparatus would not allow them to give anything else.

With regard to the apparatus, and the various nice conjuring appliances to send children off without their minding it, he thought it was kind to be cruel, and send patients off as quickly as possible. It should be remembered that the parents were not the patients, and it was the patient's feelings which must be considered, and not the parents'. With regard to fatty heart, he did not think the argument that they could give children chloroform because they had not fatty hearts would hold water, because the deaths from chloroform did not occur from the direct effect upon the heart, but from the toxic effect of the anæsthetic upon the nervous centres in the medulla. The anæsthetic paralysed the respiratory or the cardiac centre straight away, and the patient died. The death in such cases was not caused by heart failure.

With regard to shock, he had tried full and large doses of anæsthetic, and he had also tried smaller doses, but he was not at all satisfied. He knew he had kept patients thoroughly under all the time, but afterwards they had suffered from very great shock. He remembered one case in the eye department. When the patient was thoroughly under chloroform the optic nerve was divided, and that patient practically died, but was brought round after a long time by means of artificial respiration. Whether the deaths that occurred suddenly on the commencement of the operation were due to shock, or whether the anæsthetic had attacked some nervous centre which governed other nerve-centres and so allowed the shock to be more pronounced, he could not say, but he thought there was some peculiar nerve mechanism present which was not understood.

Mr. BAKEWELL said he agreed with what Mr. Willett had said regarding chloroform for young children, and entirely differed from Mr. White. He had tried ether for a good number of cases at Great Ormond Street Children's Hospital, but he had given it up as a routine method for many years. First, children struggled a good deal with the mask when gas and ether were given. Again, if the operation were a long and severe one the

children got a great deal of shock, and there was nothing to fall back upon when ether was given in the first instance. His routine method of giving anæsthetics, especially to children under six years of age, was to begin with chloroform, and if there was the slightest indication of shock to give a little ether. With regard to the question of shock in patients who were not quite under the influence of the chloroform, that depended upon the operation. If the operation were a long one he thought the patient should be got thoroughly well under. If the operation were a short one, such as a circumcision, he thought it was dangerous to give the patient too much chloroform at the beginning. If they waited until the patient did not kick, the patient might be lost. He had seen one patient upon whom circumcision was being done very bad for that reason, the patient having got beyond the dangerous zone. If the patient were allowed to have slight movement there would be no trouble. In chloroform he thought there was very little danger, because even if breathing stopped children came round quickly under artificial respiration. He thought that was why chloroform was so much safer in children than in adults.

Mr. McCARDIE said he could not get to the meeting soon enough to hear the whole of Mr. Willett's paper, but he was very pleased with the part he did hear. In giving chloroform to children he had noticed that difficulties occurred in inverse proportion to the age of the patient. He had always had full confidence in giving ether to children, even to infants not older than a few weeks or months, but he never felt the same degree of confidence when giving chloroform to them, but rather the reverse. In the case of a child they were dealing with such a small organism, and giving such a potent drug, and the dosage had to be so small that he always preferred to give ether whenever possible. He knew that the experience of some was greatly against his view. At the Children's Hospital in Birmingham there had been 15,000 administrations of chloroform and only one death, which of course was a very strong argument in favour of chloroform. The use of ether at that hospital was practically unknown. His own practice in giving an anæsthetic to small children was to first "fuddle" them, as Mr. Pridgin Teale put it,

with A.C.E. mixture, and then follow that up with ether given by the drop method. With patients up to two years of age he just dropped the ether on a wire mask, as he would chloroform for an adult. In children of two to four years he first "fuddled" them with A.C.E. and then gave them ether in much the same way with the addition of two or three folds of towel over the mask. To children of four or five years of age he gave gas and ether in the ordinary way, and had had no reason to be dissatisfied with the results. It seemed to him that in all short operations likely to be attended by shock or hæmorrhage, such as osteotomy, circumcision, and operations for adenoid growths, it was imperative to give ether. If the operation was to be a long one he thought ether should be given first, and then, after fifteen to thirty minutes, chloroform. He found these methods to answer every purpose, and thought they could be used with perfect safety. In small children even nitrous oxide gas and oxygen could sometimes be given, and even for such delicate operations as those on the eye. Only the other day he gave gas and oxygen to a small child of four during an operation for strabismus. The result was perfect. He managed at the outset to get the confidence of the child. The surgeon remarked afterwards that there was less congestion of the eye than there would have been with chloroform. He thought there was an opening for the use of gas and oxygen even in small children, especially in many short operations.

Mr. NOBLE said that he had been using a mixture of equal parts of chloroform and ether for children practically ever since he started giving anæsthetics, and from it he had always had fairly good results; better than from chloroform. That afternoon he had had the case of a child aged six months, which was brought into the hospital with an intussusception of four days' standing. It was in a very bad condition, having a quick, irregular, feeble pulse. The operation lasted three quarters of an hour, and the anæsthetisation was by equal parts of chloroform and ether. The whole of the intestines had been got outside the abdomen. He certainly rubbed the child's gums with brandy during the operation, but at the end of the operation the pulse was better than at the commencement. He would not have cared to have given chloroform in the

case. He had no experience with ether alone in very young children.

With regard to shock, he had not seen shock due to an insufficient amount of anæsthetic. He had, of course, had cases of shock in children, but he had attributed it to his having given too much anæsthetic, or else to the length and severity of the operation, or to the fact that the child was in a very bad condition before the operation was commenced.

Mr. CARTER BRAINE said he had not yet tried equal parts of chloroform and ether, as recommended by Mr. Willett, but after what he had said about it he (Mr. Braine) was inclined to try it. Up to the present he had been very pleased with the A.C.E. mixture for young children. Hearing from Mr. Willett the age-limit he intended to fix in his paper, he (Mr. Braine) had looked through his case-books in order to determine the percentages of the different anæsthetics he had administered during that same period of life. The result of that search was somewhat as follows, the period covered being the last twelve years.

A.C.E. mixture 51 per cent of the cases, the youngest patient being aged twelve days.

Ether 32 per cent., the youngest patient being aged four months.

Chloroform 17 per cent., the youngest patient being aged four months.

He had not been at all pleased with chloroform for very young children, but with the A.C.E. he had always felt perfectly comfortable and at home. For very young children, up to two or three years of age, he had generally administered the A.C.E. mixture on a Skinner's inhaler. For children beyond that age he had used a Rendle's mask. In several cases he had administered nitrous oxide and ether, but he did not think it was suitable for children of less than four years of age. If the operation was likely to be prolonged he changed to ether, also on the Rendle's mask, and he had found it to answer very well. With very young children indeed—infants of a few days old—it was always very difficult to him to tell when the child was sufficiently anæsthetised for the operation to be commenced. He had never been able to tell without putting the child to the test of pain, that is to say by nipping the skin rather severely to see if there was any

movement in response. He did not know of any other indication as to when the operation could be commenced, and would be glad of any information on the point. He knew it had been pointed out that in the case of small children if the administrator's finger was placed in the palm of the hand the child would grip it and keep it tight, and that as the anæsthetic was proceeded with the child's fingers would gradually become relaxed, until finally they became perfectly limp, and could be straightened out without the slightest force. He had tried that method, but had not found it invariably satisfactory.

Dr. FOWLER said that, as a general practitioner, he was exceedingly interested in the subject of giving anæsthetics to young children, because that subject had to be dealt with to a considerable extent. He felt that the more he attended meetings on the subject the more puzzled he became. Some great authorities said chloroform should be given, others said ether should be given, whilst another advocated chloroform followed by ether, and still another ether followed by chloroform. Therefore it seemed to be a question as to which was the strongest argument used. He heard Mr. Rowell's paper before the Harveian Society, after which he thought perhaps he had better give up chloroform, as danger might come some day, and that he should go on with A.C.E. and ether. Yet, somehow or other he came back to chloroform, particularly for very young children. He felt that in very young children, who had very tender mucous membranes, ether was likely to be very irritating to the bronchial tubes, and likely to cause trouble afterwards as well as at the time. There was a preparation of ether now which was not so pungent as the usual kind, and that might turn out to be more satisfactory for children. At the same time, chloroform acted well, and the patient could be got under very much more quickly. With regard to shock, should not they consider that no one knows what pain or shock means to another? He believed some people really did not know what pain was as others felt it. Some time ago he amputated a finger for a ward servant at the London Fever Hospital. She did not know what pain was, for though she had no anæsthetic she experienced no sensation of pain during the operation, and said she had

never done so, although several minor operations had been performed on her. In the case of others, the least prick with a needle was sufficient to produce extreme pain and discomfort. He believed the same held good with regard to shock, and he thought that those who were sensitive to pain would be also sensitive to shock, and they could not estimate what a particular individual's feelings were in that respect before an operation. In rectal operations, for instance, there was a stage at which the patient felt acute pain; it was better to go on with the anæsthetic very fully until that was reached, and then drop it as soon as possible afterwards.

With regard to the child's parents, of course, in private practice, one might have the greatest difficulty in that way, but he would not let consideration for the parents interfere with his dealings with the patient. The parents were very much better out of the room altogether, and he believed there was seldom any trouble in getting them away from the scene of operation. Then, where a pocket-handkerchief or a Skinner's inhaler were used, the child was soon under, particularly if chloroform was the drug used. If the child cried, so much the better, for then it would get quicker under the influence of the anæsthetic.

Mr. CROUCH explained with regard to his remarks about getting the right side of the child's feelings, that that was necessary, because the particular operation might be the precursor of others, and therefore it was desirable to avoid alarming the child.

Dr. DUDLEY BUXTON thought Dr. Fowler had brought out a very important point when he referred to the extreme confusion existing on the choice of anæsthetics. But the solution of the difficulty was, that no rule could be adopted for one set of cases or for any particular class of cases. The only successful plan to adopt was to regard every case in which an anæsthetic was given to a child as a separate problem, which should be thought out (the process occupied a few seconds), and then the anæsthetic given according to the solution at which the anæsthetist had arrived. It must not be said that one should always give chloroform, or should always give ether; or that one should always begin with ether and then go on to chloroform,

or begin with chloroform and then finish with ether. Such a rigid line of procedure would be likely to lead into lamentable trouble. Certain points in Mr. Willett's paper suggested comment. With regard to shock, a very important point in the ætiology of shock had been overlooked. He referred to the cooling of the patient's body when under an anæsthetic and subjected to surgical procedures. The more anæsthetic used, the greater the fall of body-temperature, and consequently, the less the resistive power of the child to loss of blood, etc. "Fatty heart" had been so often spoken of and alleged as a cause of fatality without definite evidence of its causal relation with chloroform accidents that it was almost a stock phrase. He (Dr. Dudley Buxton) thought fatty heart, so called, was often a bug-bear alarming people, but which often had very little bearing on the case. At all events, the "fatty heart" was only one factor in the production of death under chloroform, or, as he would prefer to put it, in the production of shock under chloroform. He took it that shock was an interference with the vaso-motor mechanism of the circulation. When the heart was out of gear, the vaso-motor mechanism was also out of gear. He could not follow those who said that they could entirely protect their patients from surgical shock by producing profound anæsthesia by means of chloroform. He believed the patients might be protected from certain reflexes by a fairly profound anæsthesia; but if they obtained that anæsthesia by means of chloroform, he submitted that they were bringing into the question a fresh condition of shock by the well-known interference with the vaso-motor mechanism which occurred under that anæsthetic. He supposed nobody would deny, with their present knowledge of the effects of chloroform, that it did produce profound changes in the vaso-motor mechanism of the individual, and he ventured to further submit that that was very much accentuated in the case of children. He had observed in his own practice, and had been told of many instances by others, that the vaso-motor action in children was extremely unstable, that it was far more easy to produce severe vaso-motor shock in children than in adults. For example, it was known that blushing or pallor in a child was more easily produced than was the same pheno-

menon in adults, and these phenomena were certainly due to vaso-motor changes. Then, he took it that in giving chloroform to children they were acting upon their weak point, their vaso-motor system, and were adding to the surgical shock, the shock which was inseparable from the giving of chloroform to anybody. When chloroform is given to one of the lower animals, all must be impressed, if they took the trouble to take a tracing of the blood-pressure, by the extreme amount of vaso-motor action of shock produced by the anæsthetic, even when no surgical interference had been allowed to complicate the result. So he thought they could not believe, in the present state of knowledge, that chloroform, as such, would lessen surgical shock, but on the other hand they must believe that it increased the surgical shock. He did not think that was at all a contradiction of the position which he held, namely, that fairly profound anæsthesia prevented shock by preventing reflexes due to pain, or due, as Mr. White had said, to the blocking off of the protective centres in the cord and brain, and so protecting the vital centres presiding over respiration and circulation. Of course it was essential in every case to prevent peripheral stimuli causing reflex inhibition of respiration or reflex inhibition of circulation, through the action of the heart or through the action upon the large plexuses in the abdomen.

With regard to the routine method of giving anæsthetics to children, he reverted to what he ventured to bring forward in his opening remarks; that they must decide what method to adopt, according to the condition of the child to be dealt with. Certainly very many children took gas extremely well, and ether also when given in succession to gas. Where there were after-effects following the use of ether in short operations, these were due, in his opinion, to the fact that people did not remember that ether, like other potent drugs, would produce effects varying according to the quantity given. If in the case of children they limited the amount given, he thought in ninety-nine cases out of a hundred they would not have after-effects. This was shown many years ago by Mr. Warrington Haward and Mr. Pollock, the latter gentleman having been the first to bring to the notice of the profession the extreme value of ether in

the case of very young children. A.C.E. answered very well, and better in weakly children than chloroform. He thought another point was frequently lost sight of, that chloroform was not necessarily a safe anæsthetic for children. It had been asserted through the last fifty years, like the whisper of Midas through all the ages, that "chloroform never kills, and never killed children." Unhappily the converse of that had been shown over and over again. Children possibly did not die from the effect of heart failure, but they did die from other causes, and one of those other causes was, he ventured to say, vaso-motor failure.

With regard to the particular mixture advocated by Mr. Willett, it had been used very largely abroad, and was a most valuable mixture. It could be used with an Allis's inhaler, and it could be used and the proportions of ether and chloroform varied according to the phenomena which the individual exhibited. Sometimes equal parts of chloroform and ether could be given, at others more chloroform, and at others more ether. This one inhaler would carry the administrator through the whole operation.

Another very important practical point was that even for long operations they should remember that a child who was once anæsthetised required extremely little, if any, anæsthetic to keep in the anæsthetic sleep. If the anæsthetic was kept up by giving more of the drug, after-effects were produced, which were due to the anæsthetist and not to the anæsthetic. Those things could be guarded against, and in the hands of skilful men were generally guarded against. With regard to shock in particular operations, his experience was that in eye operations the most profound and alarming shock was produced under chloroform, but he had never seen any dangerous shock effect under ether, although he had, by the employment of ether with oxygen, produced a far better anæsthesia than he would venture to attain by the use of chloroform.

(To be concluded.)

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WITH DR. SAVAGE AT BETHLEM ROYAL HOSPITAL.

GENTLEMEN,—To-day I purpose to introduce to you the subject of insanity, to point out to you the conditions under which people are placed in asylums, and the forms which mental disorders generally assume. First of all, you understand that this is an asylum, that the people who are here are here against their wills, that though a few are admitted as voluntary patients, the majority are here on compulsion; and it will be for you to learn during these lectures what patients should be sent to asylums and what patients should not; what patients may possibly be sent to hospitals, what patients may be sent to private nursing homes, but particularly what patients must be sent to asylums. Remember, every medical certificate which is signed assumes two things:—that the person is a person of unsound mind, that is factor No. 1; factor No. 2 is that the patient is a fit person to be detained in an asylum. So that it does not matter a bit in the world what a man's peculiarities—insane peculiarities if you like—are; if he does not render himself objectionable to society there is no particular reason why he should be detained in an asylum. Well, I have to point out that there is no absolute dividing line between the degree of insanity (unsoundness of mind is a better term) that requires a man to be sent to an asylum and the degree that renders it not necessary. Let us take an example. A man was brought into my consulting room about two or three weeks ago, who at once began by saying, "I am brought to you as a nerve specialist. I know there is something wrong with my nervous system. I had a serious hunting accident. I had a fracture in my hip joint; it was compound, a very serious thing indeed, and for months I was on my back. Unfortunately, since then there has grown an adhesion between the skin over my buttock and the bone, so that my life is rendered intolerable by uneasy feelings there, and you shall see the

visible effect." I went into a waiting room for a moment, and came back to my consulting room to find him stark naked. He at once set to work to demonstrate, as he thought, that there was an adhesion between the skin over the gluteal region and the head of the thigh bone, and continued to seize his gluteal muscles and pull them apart. Such conduct did not matter in my consulting room; but it was extremely inconvenient, when the family were entertaining people, to find him suddenly rising and setting to work to pull his gluteal region away from the bone. His behaviour was inconvenient, but not dangerous to society, and did not necessitate certification. Another example is that of an elderly clergyman who believed his clothes were always wet. It was an inconvenient thing for this old gentleman to have to take off his coat whenever he got an opportunity to air it in front of a fire; or, having a stove in the vestry, whenever he went there, between prayers or before or after the sermon, to take off his cassock and air it. Yet I hardly considered that alone as insanity needing asylum care. Men of such a degree of unsoundness of mind are undoubtedly not reasonable, but to a very great extent their peculiarities are based on physical trouble, or are exaggerations of a faddy habit. I was told that this clergyman, from his student days at Oxford, was always worrying about his under-things whether they were damp or not, and feeling the sheets to see whether they were damp. Here, then, we have an exaggeration of a simple fad which might be called unsoundness of mind. But was it necessary that such an one should be sent to an asylum? I know that there must be a legal dividing line between those who must be sent to asylums and those who need not, which has to be very definite; but we must depend on more than symptoms, for symptoms which in one person do not require the person to be sent to an asylum may require another to be so sent.

Take two examples, one of which I have so often referred to. A very wealthy man had no recent memory at all. He had been a fine musician, and having perfect memory of everything which he had learnt up to a few years before I saw him, as a musician he was very skilled, as a linguist he was accomplished, and he was an authority on genealogy and pedigrees. But of

recent impressions no memory at all remained. If I put a piece of music before him that he had never seen before he would say, "Difficult piece of music; I do not know whether I can play that, but I will try;" and, like the accomplished musician that he was, he would play it carefully. The music was removed, and in a minute or so he was asked, "Do you mind playing me something?" He would reply, "I will see; but I have not played for weeks." The same piece of music was put before him; he gave the same look, made the same remark, and played it in the same way. That man of course was of unsound mind. He had an enormous fortune. He knew his memory was affected, and he was quite willing that other people should administer his affairs. He lived for years on one of his estates, went out for the same walk round the house day after day, morning and afternoon; he interfered with nobody, and nobody interfered with him. He always had the pleasure of beginning the same book each day and getting as far as the twentieth page, and it gave him equal satisfaction. That man did not require to be sent to an asylum. But, on the other hand, there was a patient in Bethlem Hospital whose memory was equally defective, who had to be brought to the asylum because she had no memory and no money. There was a difference first of all in sex. Here was a middle-aged woman of good appearance, believing herself to be younger than she was, and believing herself to be attractive. She had no memory whatever, and would have been a prey to anybody who said "Come to me; I am your friend." She would say, "Oh, yes, I am your friend." I used to try practical jokes—I mean scientific experiments—upon her. I would take her into a room and say, "What month is it?" "Well," she would say, "there is no fire, the grass is green, I suppose it is spring." We would walk her out of the room, light a fire, pull down the blinds, put the clock at five o'clock, and then say, "What month is it?" She would say, "There is a fire, it is five o'clock, it is dark, I suppose it is November." She reasoned perfectly upon her partial information. That her memory was exceedingly defective is seen from the following. While in the hospital her husband died. I was asked "Should we tell her her husband is dead?" "Certainly. Why not?" "Will it not seriously

affect her?" "No." A friend began by beating about the bush and said, "I have to tell you very sad news." "Really." "Very sad news; your husband is dead." Then they thought what a foolish person the doctor was, for she burst into a storm of tears. But all at once stopping, she said, "What am I crying about?" And so once more she was told that her husband was dead. Again she burst into a flood of tears, to be instantly dried, while she asked what she was crying about. There, of course, is another example of absolute defect of recent memory, and yet one accepted her as an inmate of Bethlem Hospital because, when she was admitted, her husband was an invalid, and had no means wherewith to provide any one to look after her; and, therefore, being of unsound mind, she was obliged to be considered a fit and proper person to be detained in an asylum.

Remember that in regard to every patient that comes before you, you have to decide not only as to the unsoundness of mind, but the fitness of that patient to be under control. Take two examples. I have seen this morning in a nursing home a young lady who arrived yesterday in London, who talked to me perfectly reasonably and nicely. A point you must remember is, that while sometimes the symptoms show themselves at once, and you have no bother or hesitation about the line of examination, at others you have to wear down the defences before getting to the real mental state. After talking for some time with this girl about her journey and about things that had happened, and about her relations, and her home, her music, and her art, and all the rest of it, I asked her about sleep. "Ah, there's the difficulty. You know I am disturbed at night. I do not know why it is, but wherever I am there are noises which I do not understand, and they all seem to refer to me. It is some one who seems to know more about me; they are so liable to talk loud; and people seem to lead my thoughts, to influence me and inject their own." Now here I had a very definite clue. (Q.) Were you also disturbed sometimes by seeing things? (A.) "Yes, and that is funny you know. And this is an important thing: when I am half awake and half asleep in the morning, my room seems to be full of people's faces doing all sorts of odd things. I do not know how they

come into my room." Here remark the time for watching and noting symptoms of mental disorder is the early morning. The suicidal patient is most suicidal in the very early morning. A gentleman whom I also saw to-day is a very good example of the kind of dividing line between the patient who should be sent and the person who need not be sent to an asylum. Some eighteen months ago he brought his wife up to London to see me. Unfortunately for me I was out of town, and she was seen by somebody else, and was allowed to go to a hydropathic, and from there was allowed to go somewhere else and hang herself. Her husband had been told to leave her. The day he left her, two or three days before she hanged herself, she said, "Good-bye, but why are you going?" He said, "I am told to go." She said, "I wish you were not going." He went. She committed suicide, and from that time there has been one echo all through his life, "Why did you go? If only you had not gone." He said, "No sooner do I put my head upon the pillow than the voice says "Why did you go?" Life to me is almost intolerable. I do not fall off to sleep for a long time, but I awake at two or three o'clock in the morning, and then the misery is intense and extreme, and I feel I must have some one near or I shall do something to myself." Here the man fully recognises the cause of his trouble; he has lost command of his thoughts, as it were, and there is one idea echoing through his brain. Of course he is not of absolutely sound mind, but it would be rather hard to sign a certificate of insanity for him. He knows everything, and he knows the danger to himself, and he is quite willing to guard against that danger. I said to him, "Of course you must not be alone, and I should not recommend you to go for a sea voyage, as some friends have." The sea is a dangerous place to send suicidal patients to. It would be very good if you had a kind of torpedo netting around the ship they are to go in; but I think for these patients, or even for ordinary melancholic patients, to be sent to sea is risky and dangerous. Of course in a case like this, the man being reasonable and accepting the symptoms, such as they are, says, "Very well, I am prepared to do what you advise, and it will be done on your responsibility." If a doctor, how-

ever, allows such a patient to live with him he incurs a certain amount of risk, because if such a man commits suicide the British jury will find that the man was of unsound mind when he did it, which will give an opening for the Commissioners to prosecute the doctor who received a man who was virtually of unsound mind when he was dwelling with him; for you know, or will have to know sooner or later, that you must not receive a patient of unsound mind into your house for profit without his being certified. The law says you must not have a person who is a lunatic; and at present, unfortunately, the words of the Lunacy Act are, "A lunatic is a person of unsound mind or an idiot;" which seems to imply that any one who is of unsound mind ought to be certified. As I am constantly protesting, and shall protest elsewhere more definitely, those persons are not necessarily insane who have some unsoundness of mind, such as is exemplified in cases of want of memory.

I referred in a lecture to a man who was in Bethlem as the result of the overgrowth of his profession. He was a lawyer, and a very careful lawyer. He was always weighing *pros* and *cons* until he got into a condition in which he could do nothing but weigh the *pros* and *cons*, and decide on nothing. In the old days I used to ask the house physicians to invite this patient to lunch. They did so, and I went to visit them an hour afterwards when they were still trying to persuade him to sit down. After arguing for perhaps half an hour, they persuaded him to go from his ward to their room, and when they got him there he would bring forward arguments as to whether he, being a patient, and a portion being provided for each patient by the Hospital, was not acting wrongly in lunching somewhere else or having food from somebody else. Being satisfied that this did not matter, he questioned who had the right to ask him, and whether he had the right to accept, as he was a person certified with no legal will or power to act, and, being legally dead, was not in a position to accept an invitation. So it occupied the students the rest of the afternoon in getting their own lunch and inducing him to take some. A lesson like that is never forgotten. Here was a man who would talk about anything else as reasonably as you please. As a man of unsound mind he had to be treated as

a lunatic, but I see a great many connecting links in men who cannot make up their minds. In another lecture I referred to a medical student who was at Edinburgh. He was always making resolutions that to-morrow he was going to commence steady reading. It was always "to-morrow" he was going to make his effort, and he tried all he could to stimulate the will. When he got towards the end of the week, Thursday or Friday, he would say, "Next week I will begin in earnest." Monday morning found him in bed at ten or eleven, not having commenced. Then he would say, "I will punish myself," and accordingly he would burn a little spot in his hand. At first he burned himself with ordinary wooden matches, then with wax matches, and later he got as far as using Vesuvians, and there were several severe holes which he had burnt as stimuli to his will.

There are lots of patients ranging from the person who is suffering from indecision to the person who is actually insane and suffering from what has been called the folly of doubt. I must point out in the same way that there are defects of self-control that are not actual insanity, but I regret to say are troublesome in many ways. For instance, a person about whom I was recently consulted has been a morphia taker for thirty years. Well, she goes on taking morphia; she knows it is wrong, she knows it will gradually undermine her constitution, that her memory is failing, that her power of business has gone, so that she has to get others to do it for her. Yet the desire for rest and satisfaction is too strong for her. Thus defect of self-control may merely lead to bad habits, as in this case, or as in the case of a lady, whom I regret to say I was called to see after midnight last night, whose defect of self-control showed itself in drunkenness. Unfortunately though this is a condition of unsoundness of mind, yet it is not a condition which you can justifiably certify as insanity, and this is another very important point for you to bear in mind. I hope that by the time you men are in practice that there will be some alteration in regard to this subject; but remember that although alcoholism and the acute results of alcoholism produce unsoundness of mind, you must not treat them as cases of insanity, or you will have grounds to regret it. The only legal troubles I have had

in my professional life have been dependent upon such cases. Let us take an example.

A man was admitted into Bethlem Hospital on two certificates stating that he was both homicidal and suicidal. There were many facts confirming these opinions, such as that he had written an account of his own suicide, and his own obituary notice was found upon him. He had a loaded pistol, for which he had no legitimate use, and he had a bowie knife, as well as a packet of rat poison, and, as far as he was concerned, the worst of all, a bottle of gin. He had also said he was going to kill his wife and family and then kill himself, and he left directions that the obituary notice was to appear in a local paper. On the strength of a certificate indicating these facts he was admitted into Bethlem Hospital. What more dangerous man to society could you have. Yet after eight hours the drink was out of him, and it was a question what was to be done with him. I treated him with every kindness and consideration, and promised that he should go to the Convalescent Home, and I tried by every means to persuade him to rest for a time in the hospital, but he declined; and then, he being rather troublesome and being as sane as he ever would be, we were obliged to let him go. And he devoted the next year or two to malignant prosecution of the hospital authorities and myself. His pleas were that I should never have received him, that I should never have detained him, and that I certainly ought never to have discharged him. This prosecution went to the highest courts, and finally to the House of Lords, and cost the Hospital on my behalf £800. So you see, although a person is of unsound mind and even insane, you have to use your judgment whether you should sign a certificate for his removal to an asylum. One of my first pieces of definite advice is, "Never sign a certificate for a person who is drunk at the time, nor for one whose symptoms are almost certain to pass off as soon as the drink is gone." There are persons who are absolute lunatics as a result of alcohol. For instance, I have seen to-day a man who will very likely soon be in Bethlem Hospital. He has been intemperate, and is one of the noble army from Kimberley, where, at all events, he had not much chance of drinking; but he had done a hard lot

of it before he went out, and now, with recent strain on an over-stimulated nervous system, he is showing signs of insanity. With drink continued for a long time you may get a perversion of the sense—which is permanent. A patient may have delirium tremens, in which state it is not a wise and proper thing for you to send him to an asylum. If he is a poor person send him to the infirmary for the safety of himself and family, but do not certify him as being insane. But if a man has drunk until he is in a chronic state of delirium tremens, so that he is always seeing things that do not exist, feeling electrical shocks and believing that he is being electrified, influenced, galvanised, paralysed, that parts of his body are being removed, that his inside is being tampered with, then you may send him to an asylum, though the symptoms have been produced by alcohol. In the same way certain weak-minded people, such as the cases I referred to before who have become weak-minded through alcohol, and having lost all recent memory, if there is no one to look after them, will probably have to be sent to a county asylum.

We will now have in one or two exemplary forms of insanity. This initial talk has been to make clear to you that there is grave responsibility in certifying; that in certifying you must be sure that a person is not only of unsound mind, but is also of unsound mind to such a degree as to require, for his sake and for the sake of society, that he should be detained for proper care in an asylum, or in a private house. And you also understand that you cannot, in a benevolent sort of way—benevolent to yourself or to the patient,—receive a patient into your house who is a certifiable lunatic unless he is certified. You may of course convert a patient's house into a private asylum. Now and then one is called down to see some very wealthy patient. Take for example:—I was asked to see the wife of a very wealthy man, who has a house with two wings, and the man insisted that the case should be treated at home. My reply was, "Very well, if you have nurses and a resident doctor this may be done, though it is a question if it is the best thing for the patient."

Now notice this patient suffering from simple acute mania. The first evident characteristic is defect of higher self-control, a condition of con-

tent and satisfaction. Most evident of all is defective control of the emotions. She is buoyant and boisterous. The general aspect and the appearance of the patient have to be noted. There is a general disregard of social restraints. I often say a patient is no better than her hair; that a female patient who has lost her higher self-control is untidy in hair and dress. The natural instinct of a cultivated woman is at least to look her best in the matter of her hair, her complexion, and her dress, and in everything else she should be conventional and in order. One of the first effects of loss of higher self-control is disregard of little things. Then there is restlessness and an inability to fix themselves to one thing; next is a general tendency to say at once just what they think; with them speech, not silence, is golden. As one has so often to point out, the higher cultivation is associated with the masking of one's feelings and suppressing one's true thoughts. The lower patients are in the social scale, the more readily do they say what they think. The germ of development in society is that people only half reveal the thoughts that they have. Our patient here says there is no doubt she is mad, but she feels very happy about it. The important symptoms you have to notice are the evidences of defective higher self-control, and this defect shows itself in different ways in the two sexes, and in men and women of different ages, but there is in all a disregard of the conventional. So much for the mental side.

Next as to the physical side. There is too much unrest for acutely maniacal patients to be well nourished. They are generally thin, generally pale, but the appetite as a rule is good, even large, though sometimes capricious. Circulation as a rule is fair, the pulse tending to rapidity and weakness. This patient has rather a weak than a strong pulse, and that is the case in mania, notwithstanding the restlessness and all the apparent physical energy. In women there is not infrequently metrorrhagia or irregular menstruation. You often meet with erotic excitement in such cases. Loss of self-control means not only self-control in the conduct, but also in the emotions and passions. This case represents one of the forms of mania. The other forms will be pointed out at some future time.

This patient shows the form of mania chiefly exhibited by defect of self-control. There is a too rapid reaction (uncontrolled) in such cases. One patient, when I took out my watch, said at once, "Gold, untold wealth, ring, fingers, marriage, engagement." You can trace the sequence of ideas, the verbal and intellectual association. Or take another example which I have often mentioned. Dr. Wilks came to Bethlem and saw a patient dance round who, on hearing his name, at once said, "Wilks, whelks, oysters, shell fish." Here, again, we have laid bare the association of ideas. That is the way a maniacal person reacts to a sensory impression. Very often you get apparent incoherence as a result of it. Thus whilst they are running along one line of thought, due to one sensory impression, another impression comes from another sense which interferes with it; so you get the two running together and, as a consequence, mixed associations. I once put my hat on the bed of a maniacal patient, when suddenly, as rapidly as possible, she said "kitten." She does not come directly to kitten, but the sequence is "hat, cat, mouse, trap, kitten." First verbal, then intellectual association. There you get a type of disorder which is so constantly met with in mania, a too rapid reaction to impressions. The impression is made and is acted upon at once; then some other thought is associated with another sensory impression which gives rise to some idea, and this is brought into relation with the former idea, there being no visible link between the two.

The next case is one of so-called acute hysterical mania. It is that of a young woman who has gone through a period of sleeplessness and depression, followed by reaction, during which possibly her friends thought she was perfectly well. But she loses all her self-control, and becomes mad. During this period of excitement she does exactly what comes into her mind. If it strikes her to break a thing she breaks it. She has no malice. I sometimes say this condition is one of destructive constructiveness. Such patients often tear up their clothes. She may say, "This is not a nice dress, but I could make it nice if I did it up myself." She gets as far as tearing it up, and by the time she has done that her ideas have changed. So in many cases ideas of destruction come into their minds, which originate in the

thought that they are going to improve things. Many of them destroy things with the idea of beginning again, but there is a want of continuity in their efforts. You cannot say what is going to be the result of an attack of insanity of that kind. The patient may rapidly become better, passing into a condition of nervous and mental exhaustion, and then recovering.

It is common to make use of the terms Melancholia, Mania, and Dementia as representing different forms of mental disorder that we shall have to consider separately. But the truth is that almost every typical case of acute insanity goes through all these stages or forms. Nearly all cases of mania are preceded by periods of depression followed by periods of excitement, and periods of excitement followed by periods of exhaustion; so that you get the melancholic stage followed by the maniacal stage, and the maniacal stage followed again by one of weak-mindedness. There are certain cases that one could point out that never get beyond the maniacal stage; there are certain others which never get beyond the melancholic stage, or at all events there are cases in which the maniacal or demented stages are most marked and definite. In some these states of disorder may be the terminals,—that is to say, certain patients permanently remain maniacal, others remain melancholic, while some remain demented.

Here, then, is a different type altogether. A lady who, instead of having defect of self-control, has exaggerated self-feeling. One would say that just as the maniacal patient is characterised by that one thing—the bubbling up of ideas and emotions and actions,—so, on the other hand, the type we are now considering seems to be all centred in self-feeling. Exaggeration of feeling is the basis of these melancholic states. In this case is manifested the monotony of grief. This lady does not speak to tell us what her sorrow is about; all we can get from her is her extremely sorrowful demeanour. She looks constantly sad and oppressed with grief, worried and anxious, and at the same time she picks holes in her face and hands. We see some fundamental relationship between sorrow and grief, and biting the nails and picking holes in the face, but have not yet been able to trace it. In this patient, besides the aspect of sorrow we have the sigh-

ing; and another active sign is that she resists when one wants her to do anything. If you want her to come away from one part of the room she resists at once. When the attendants want to dress her they have great trouble; and an equal difficulty occurs with undressing. In the maniacal cases you have no very marked evidences of functional bodily disorder,—that is to say, they often sleep fairly well, they generally eat well, the catamenia are as a rule regular, digestion is fair, the bowels are not troublesome, the skin acts fairly, and the temperature is normal. But when you come to these cases of melancholia, nearly all the bodily functions are badly performed; the circulation is feeble, so that the extremities are almost always cold; the brain is feebly nourished, and the digestive organs as well, so that there is a distaste for food and frequently a refusal to take it; the bowels are almost always constipated; the senses are slow to perceive, and the catamenia as a rule are absent. No sooner do you get this lady in one position than she wants to be somewhere else; you let her be somewhere else and she wants to be back again. Feelings of unrest appear both here and in the maniacal conditions, but at the same time they are different. The aspect is very different. As I have said, the maniacal person who has defective self-control or defective higher control shows it in different ways according to age and sex. The woman becomes typically hysterical or emotional when she loses the higher control. A man frequently becomes morose, brutal, and aggressive; he is a much more trying and troublesome person in that way than women. In the same way with melancholia, a man as a rule is much more concerned about himself. A woman, when she is depressed, thinks much more about others. The man is much the more selfish animal, and it is shown very markedly in melancholic depression. The men I see most frequently with melancholia are concerned about their money, or their digestion, or something of that sort. Women, on the other hand, are wondering whether they have done their duty, whether they have not neglected their children, whether they have not neglected their husband, whether after all they have not been shamming. The essence of melancholia is the feeling of depression, which is associated with

feeble bodily nutrition. The form of delusion will depend to a very great extent upon the age and sex of the individual. An old man or an old woman almost always fears the workhouse. The middle-aged man is afraid of losing his business, his position, his situation; afraid that people will not trust him, that they have begun to doubt his stability and honour. A woman begins to think she is in the way, that she is no longer what she was to her husband, no longer what she was to her children; she thinks they have begun to believe that they can do without her. Sleeplessness is one of the characteristic symptoms of mental depression of this kind. A badly nourished brain is associated with sleeplessness; so you see sleeplessness is a symptom of bad nutrition, which is constantly met with in these cases. In a person going through this melancholic stage, the chief thing you can do is to see that no harm happens to them from without or from themselves; see that they are properly nursed, properly fed, and not able to injure themselves. It is of no use for a patient like this to try to dissipate her misery. You can hear somebody say, "I went to see my sister in the asylum, and they did not do anything to amuse her." Fancy trying to amuse a patient like this. It cannot be done; they are altogether outside amusement. In the same way a lot of persons in this condition are sent to be cured by travel. That is not the best thing for people who are suffering from melancholia. People who are ill in body, and as a consequence are ill in mind, require to be nursed; therefore I say emphatically that melancholic patients are very much better in bed than voyaging. There may be too much bed occasionally, but I am sure there is far too much attempting to make such patients travel. But people say, "Surely it does them good to stir them up." It does not. I constantly say to people who want to stir up their friends when they are melancholic, "When you have a bad eye, do you think you are wise in going the round of the picture galleries to stir the eyes up?" They answer that the patient is mad; but I say he is no more mad than those who advise melancholic people to be stirred up by travelling. They have to go through the same process as a typhoid patient, or a pneumonic patient, and you cannot hasten it very well. You

may retard it, and you may interfere with it, if you do too much. I would point out to you in bringing before you cases of mania or melancholia that there may be temporary functional disorders associated with some trifling physical disorder, some trifling shock, some trifling trouble, or they may be the signal of some serious illness. Therefore you must not consider you have done very much when you say "Here is a case of mania." You have to remember that mania is a form of disorder which may be associated with brain disease, may be associated with some temporary malnutrition of the brain. Melancholia, in the same way, may depend entirely upon some brain disorder or disease. Or it may, on the other hand, be associated with some disease of the kidney, liver, or other organ. Thus I saw a case the other day associated with myxœdema. It may be associated with some malignant disease of the stomach and liver, or such like. There is another thing to say before parting with this resistive melancholic lady. The forms of insanity, as I have told you, depend to a certain extent upon the age, they also depend to a certain extent upon the temperament. That is seen much more clearly in such cases as those of melancholia. Grief affects men differently according to their education, their age, their temperament, and the social surroundings; one person battles against misfortune, another collapses under it. So you will see one set of people with the same feeling of misery at the basis take different turns. One man says "Damn," and another man says "Thy will be done." The expression of melancholia differs with the individual. Later on I shall have to enlarge upon the forms of melancholia, as I shall have to enlarge upon the forms of mania; and as I have told you, melancholia, like mania, may be only a stage which we may trust will pass off.

The third form of mental disorder that I am going to refer to to-day is that of Dementia, weak-mindedness. Of course there is every grade of weak-mindedness, from simple loss of memory to absolute mental want, in which the individual is simply an automaton who washes, feeds, and dresses without any social qualities of any kind. Such have ceased to be social; they are, as the insane have been called, "aliens" to society.

They are indifferent to all their surroundings. Here is a patient who has been eating, drinking, sleeping, in Bethlem to my knowledge for forty years; she is one of the incurables of the hospital. When I was house physician in 1864 here, she looked vacantly about her just as she does now, and she may very likely outlive me. A period of acute mental disorder, whether it be melancholia or mania, may leave a condition of this kind, varying in degree, as I have said, and varying in aspect. So long as this patient has enough to eat and drink she cares for nothing else. She would be totally unfit to protect herself. Probably if you put her into a new position with new surroundings, where she would have to use thought and ingenuity to find the key to get at her food, she would die of starvation, being unable to adapt herself. This patient's relations have all died long ago. But if we told her that her only brother or sister had died she would simply say "Oh;" it would be a matter of perfect indifference. Mental weakness can be so pronounced as to convert the person into a mere automaton. I always think in this connection of a man who, many years ago, was here, who when he awoke in the morning would begin to say, "Breakfast at eight o'clock," and continue to do so until breakfast time, the remark being made in a loud cry as the clock got to five minutes to eight. After breakfast he would begin, "Beef tea at eleven, beef tea at eleven;" and then after that, with the same monotony, "Mutton and potatoes for dinner; mutton and potatoes for dinner." After that it was "Tea at five o'clock." His wife, a devoted little woman, came regularly to visit him, and used to bring him buns or fruit. As soon as she was seen by the patient he would say, "What have you got?" And as soon as he got possession of it, he went down to the other end of the ward and took no further notice of his wife. Here was a man who had been a kindly, considerate husband and father, an individual who had taken a deep interest in the welfare of his family, but after a mental illness had ceased to take any interest in anything but eating and drinking. I am obliged to point out that these are wrecks and ruins, and the form the ruin takes depends upon the way it was built. Just as there are picturesque old castles, so there are intellectual ruins that remain interesting to the end.

There are houses jerry-built which a very little storm reduces to uninteresting masses of bricks and mortar; and that is what the majority of chronic lunatics become. And remember that there is every variety of wreck associated with the various forms of mental and bodily disorder which give rise to the trouble.

DR. A. M. PHELPS of New York, writing on tuberculous and purulent joints, says:—It is a fact that perhaps one abscess in ten, either of joints or of any other tissues of the body, may be absorbed. It is also a fact that more than 50 per cent. of abscesses of joints will find their way to the surface in such a manner as to establish good drainage, and thereby allow the joint to recover under proper mechanical treatment. But what are we to do with the other 50 per cent. of cases that produce dangerous disturbances? How are we to distinguish the benign from the malignant? Without an exploration we cannot. No surgeon of any reputation would ever think of trusting a case of osteo-myelitis or tuberculous abscess of bone to Nature. Such conditions are always operated upon, and life and limb saved that otherwise would have been sacrificed. In cases of appendicitis no surgeon to-day would think of trusting such cases to Nature. An abscess of the appendix ruptures into the abdominal cavity, when not surgically treated, in 75 per cent. of all cases, and there is a mortality of over 35 per cent., whereas with early surgical interference the mortality is a fraction of 1 per cent. These same arguments can be applied to abscesses of the joints. If every joint which is suppurating, whether tuberculous or purulent, is operated upon, the mortality and destruction of that joint will be reduced to a very small percentage, whereas, if not operated upon, the mortality ranges from 8 to 12 per cent., and excisions or chronic sinuses lasting for years or for life will be found in a very large percentage of such cases, just what percentage I am unable to state. I know that in my clinic at the Post-Graduate Hospital, which is attended largely by patients from other clinics and hospitals, more than 75 per cent. of the patients have chronic diseases of the joints with abscesses involving the bones of the joint, and frequently of the pelvis, which have existed for years. Now, if such cases had been early examined and the abscess evacuated, the excisions which I am now obliged to perform could be avoided.—*New York Medical Journal.*

WITH DR. LEWIS JONES IN THE ELECTRICAL DEPARTMENT OF ST. BARTHOLOMEW'S HOSPITAL.

Neuritis from Disease.

GENTLEMEN,—I have spoken at some length on neuritis, especially those forms which are so common in this department of neuritis due to injuries of nerves. And I still have more to say about neuritis because it is one of the commonest diseases for which treatment is sought in this department. Neuritis nowadays, is a very well-recognised condition, but it is not so long ago since neuritis was a word which was seldom heard, because definite neuritis was regarded as something of a rarity.

To us the special interest about neuritis is that it is curable to a very large extent. It therefore differs from a great many of the diseases of the nervous system that one is familiar with, and which one recognises as almost incurable. Neuritis is essentially a disease for electrical treatment, and it is remarkable to what an extent electrical treatment facilitates recovery.

In considering the conditions which produce neuritis, apart from accident or injury, we may take first the neuritis which is left after febrile diseases, of which the type is diphtheritic paralysis. Diphtheritic paralysis varies very much in the severity with which it attacks different patients, and in the time which patients take to recover from it. Diphtheritic paralysis, as a rule,—and I am excepting now the cases which prove fatal from bulbar symptoms,—tends to recover whether it is treated or untreated. But there is no doubt that if you have a patient with diphtheritic paralysis it is not good practice to leave him untreated to recover by himself. If in the course of a case of diphtheritic paralysis electrical treatment be begun, you will see an immediate step forward in the rate at which the patient recovers. I have had opportunities of comparing the progress of cases left untreated with that of others which have been treated electrically. There is no doubt that those which have had electricity applied recover more quickly. The post-diphtheritic cases which

come here are usually suffering from a partial paralysis, perhaps affecting all the muscles of the body more or less, but especially manifest in a considerable loss of walking power, or even loss of power to stand. The treatment for these, therefore, is the electrical bath with alternating or sinusoidal current for the whole body.

A general or a local neuritis may also come on after other specific diseases, such as typhoid fever and influenza, and after other less definite febrile conditions. After typhoid fever such sequelæ are not very uncommon, while after influenza they are very common. An outbreak of influenza in this country is followed by quite a crop of paralyses, which are more usually peripheral palsies, affecting one or more groups of muscles supplied by one or more peripheral nerves. So again, some time ago we had a patient here with paralysis of one serratus magnus after a feverish illness, the nature of which was not very clearly defined. As far as one could make out he had a stricture; a catheter was passed, and after that he had shivering and fever. When he recovered from the attack it was found that his serratus magnus was paralysed. In another case a nurse contracted a septic inflammation of one hand while assisting at an operation, and this was followed by a distinct attack of neuritis in the ulnar nerve of that side.

So you must be prepared from time to time to meet with cases in which, after acute feverish diseases, peripheral palsies result, due to neuritis. The affected muscles may present the reaction of degeneration if the neuritis be severe. All such cases should have electrical treatment, for that is by far the best way of influencing the nutrition, and repairing the damage in the diseased nerve-trunks.

Next to those cases of peripheral neuritis after fevers, we may take those of poisons—alcohol, lead, arsenic. Alcoholic neuritis is usually a general neuritis, affecting all or nearly all the peripheral nerves, though some may be more affected than others; often the symptoms are most manifest in the lower limbs, and especially in the front muscles of the legs. Lead, again, may affect all the nerves of the body, and produce a general peripheral neuritis; but as a rule there are certain parts affected most, and those are the extensor muscles of the wrist and fingers. At the present time we have one patient, a lead case, attending

with paralysis of his peronei as well as double wrist drop. Arsenic is a less common cause of peripheral neuritis; in fact, arsenical neuritis is not a condition you are likely to see very often, but you must remember that sometimes arsenical neuritis is produced by medicinal doses of the drug. I can only remember two cases clearly which have been under treatment here, and one of those was a little girl, who had been treated with arsenic for chorea. She developed a general neuritis with paralysis and a reaction of degeneration in many muscles from the drug. Under electrical treatment she got well. The other was a patient who had a severe attack of general neuritis from a single dose of arsenic, which was intended to poison him. He was a warder in a prison abroad, and an enemy of his mixed some arsenic in his food, and very nearly killed him from the direct effect of the poison. But he was a big robust man, and survived the first effects of the arsenic, and while he was recovering from it he gradually developed paralytic symptoms which were believed at first to be those of locomotor ataxy, because he walked very unsteadily, and had lost his knee-jerks. He was sent home to England for treatment, in the hope that something might be done for him. But his story, as soon as he arrived, clearly showed that the arsenic had to be considered in the case. Not only that, but recovery had begun on his voyage home, and that fact helped one to form the opinion that it was not tabes. Finally, after a year's treatment, he was able to go back to his work perfectly well. The reason that case was mistaken for one of tabes is instructive. You know that loss of the knee-jerks in locomotor ataxy is a very important symptom, and it is one which most medical men lay great stress on. It is also a marked symptom in paralysis from peripheral neuritis. This patient had vague paralytic symptoms, with loss of knee-jerks, and he was believed to have tabes. What he had was peripheral neuritis, and arsenic was the cause of it.

An interesting thing which is mentioned in books in connection with arsenical poisoning, and which we noticed in him was that during his illness the toe nails and the finger nails were shed several times. During his recovery the electrical reactions returned to the normal, although there was a reaction of degeneration in many of his muscles when we first saw him.

Those three poisons form an interesting group. There are certain other chemical poisons which affect people who work in particular industries. We need not consider those here, nor the various ways in which lead poisoning may be acquired.

The principle of treatment in all these cases of peripheral neuritis is that, when the peripheral neuritis is extensive, the patients should be given an electrical bath, so as to treat the whole body. On the other hand, when the symptoms are local, as, for example, in the wrist drop of lead poisoning, a local arm bath may be sufficient. The current which answers best in both cases is the alternating or sinusoidal current, the latter being the current which is generated by an alternating current dynamo, and distributed by the electric light mains. This answers admirably for the purpose of simple stimulation, which is the principle underlying the treatment of these cases of neuritis.

Now for other less definite and decided conditions, such as gouty and rheumatic neuritis. That neuritis is produced by gouty conditions is quite certain, although the evidence is not always quite so clear as one would wish. In the case of rheumatic neuritis, the word is often applied to a neuritis of which the cause is not very apparent. A good instance of a neuritis which is often spoken of as rheumatic is facial paralysis. Another condition which no doubt is gouty sometimes, is so-called deltoid rheumatism, which might more reasonably be called circumflex neuritis. Deltoid rheumatism is a very common affection, and perhaps most of you are familiar with it. The patient complains of a pain like rheumatism in the shoulder, with weakness in the deltoid muscle, and any movements of the arm which call the deltoid into play are painful. In bad cases there is atrophy and partial paralysis, with reaction of degeneration in the muscle.

The presence or absence of pain in the various forms of neuritis seems to be a little capricious; there may be neuritis with pain, and neuritis without pain, in spite of the fact that most of the nerve-trunks which are the seat of neuritis are mixed nerve-trunks, containing both sensory and motor fibres. In these cases of "rheumatic" circumflex neuritis both the motor and the sensory nerves are involved; but it is more often a painful neuritis rather than a paralytic one, and therefore it is best treated by the continuous current.

One of the commonest forms of neuritis which

come here for electrical treatment is facial paralysis, and among the cases we have examples of all degrees of severity. The electrical testing of the muscles gives the clue to the extent of the damage, and to the probable duration of the case. You may have well-marked paralysis, with reactions either normal or showing only slight diminution in reaction, or there may be complete or partial reaction of degeneration. Cases in which there is only simple diminution of electrical reaction will soon get well under electrical treatment, while those with the reaction of degeneration will be slow. Most cases of uncomplicated facial paralysis do well. Some may recover but slowly, and a few recover incompletely. Before you venture on a prognosis you must remember that, although, as a rule, the cause of facial paralysis is a pressure on the nerve which is transient in its nature, yet it is sometimes due to other things of a different character, which may tend to be progressive, and you must be careful to modify your forecast accordingly. An example of this is when facial paralysis is due to bone disease from the ear. Such a case as this is not to be regarded as simple because the muscles at the onset may present the normal reactions. You must remember that an extension of the disease must be looked for.

I do not say anything about facial paralysis which is a part of hemiplegia, as we are now considering cases of neuritis.

You must remember syphilis as a possible cause of neuritis. Symptoms of neuritis are often due to pressure on a nerve by a syphilitic new growth.

I remember a man who came here with paralysis and atrophy of every muscle of his right shoulder. The history seemed to suggest that it might have been a syphilitic condition. But whilst he was attending he developed severe neuralgic pains in the other shoulder. Those pains got worse and they were associated with muscular weakness, which one might almost call partial paralysis. By means of treatment, electrical and other, we were able to prevent any further advance on the left side. The mischief on the right side had been so serious as to cause permanent damage, which was never completely recovered from. No doubt he had a syphilitic growth pressing on the roots of some of the nerves to the arm, permanently damaging those on one side and developing on

the opposite side threatening to produce there a similar condition, only fortunately it was stopped by treatment.

Then I may refer to a neuritis, which I think I have been able to recognise here, in women at the time of the menopause. It is very common for women at that time to complain of various vague nerve symptoms, vaso-motor symptoms which they describe as flushings of heat and cold, tingling and numbness of the upper extremities. On one or two occasions I have been able to recognise some atrophy in the muscles.

There are also the occupation paralyses which one sees occasionally, that is to say paralysis of muscles which are over-used in the occupation of the individual. Some of these, I believe, are purely due to a neuritis, though I do not say they all are. For instance, writers' cramp, which is the best-known of the occupation paralyses, is not, I think, a neuritis at all, but I do say that some of the cases of occupation paralyses are due to a neuritis.

You may sometimes meet with young adults who without definite apparent cause develop wasting with electrical changes in certain muscles, most often in some part of the upper limb, sometimes symmetrical, and probably due to neuritis. Most of these cases will require electrical treatment in addition to any other measures you may employ. The treatment which they will want in most cases is simple stimulation with the induction coil, with or without the arm bath, but you must make an exception in those cases in which pain is marked. Painful cases of course will not enjoy induction-coil applications, but they will experience considerable relief from the constant current, especially if it be applied carefully, without any sudden makes and breaks, and without reversals, with the negative pole on the painful region. In sciatica, for example, which frequently is a rheumatic neuritis, the continuous current should be used when the pain is acute. The bath or sinusoidal current, or induction coil current, is sufficient when the case is not acute or recent, for then simple stimulation seems to meet the requirements very well. But the acute and the painful cases must have the constant current carefully and gently applied.

A DISCUSSION ON THE **ADMINISTRATION AND CHOICE** **OF ANÆSTHETICS FOR INFANTS** **AND YOUNG CHILDREN**

At a Meeting of the Society of Anæsthetists.

(Concluded from p. 336.)

DR. PROBYN-WILLIAMS agreed with one or two other speakers who said that they would be sorry to rejoice in Mr. Willett's reason which he gave the students for the use of chloroform rather than ether for children, namely, simply because there was but a small chance of their having fatty heart. Mr. Willett spoke of the large ether apparatus frightening children under two years of age. He did not know whether Mr. Willett was in the habit of using Clover's inhaler for such young children. He would like to ask whether he had tried small felt cones of various sizes, each containing a small sponge. These cones, he believed, were first used by Mr. Woodhouse Braine for giving ether to small children. They possessed the further advantage of being very convenient for putting a few drops of A.C.E. on to the sponge, to start the anæsthesia, which could be continued with pure ether. In advocating the mixture of equal parts of chloroform and ether he understood Mr. Willett to say that he gave it for all ordinary operations, such as opening abscesses, circumcision, etc., but that when he had an operation for strabismus, or one requiring deeper anæsthesia, he gave pure chloroform. He (Dr. Probyn-Williams) would have thought that with a mixture of chloroform and ether, or even A.C.E. mixture, an anæsthesia certainly as deep as was safe could easily be obtained. At the London Hospital regulations were given to the resident House Surgeons and House Physicians regarding the administration of anæsthetics. These provided that if the patient was under six years of age they were to give ether, unless there was some reason to the contrary, either in the condition of the patient or in the nature of the operation. In two years, in the general wards there had been 872 anæsthetisa-

tions, and of these ether was given in about 49 per cent. of the cases, A.C.E. 30 per cent., chloroform 21 per cent. In the separation wards, for cases of diphtheria, in the first year there were 75 tracheotomies, in the second year 71. In the first year A.C.E. was given ten times, and chloroform sixty-five times; and in the second year chloroform was given in 71 cases. By adding the two years together and including all the tracheotomies, there were 1018 cases. Ether still stood first with 42 per cent., A.C.E. 27 per cent. and chloroform, which had manifestly increased, 31 per cent. There were, as far as he had been able to make out, no cases in which there was any serious trouble from the respiratory system when ether was given. If the child was old enough the concave face-piece suggested by Dr. Hewitt was used with the Clover's inhaler, but if not, a felt cone was used, or a Skinner's mask.

MR. GRANT MORRIS said that for six or seven years he had made use of the mixture advocated by Mr. Willett, and had been very pleased with the results. When he first started it he did not know of its efficacy. He had been so struck with the cases of shock which he had had in his practice and had seen in the out-patient department in the case of circumcision in tiny children that he began to wonder how it could be obviated. When he added 50 per cent. of ether to the chloroform he found a great improvement in the results, in that children kept their colour, and respiration did not seem to get so shallow. Like Mr. Carter Braine, he had found great difficulty in knowing when children were ready. He had found the corneal reflex unreliable. He would like to know from Dr. Dudley Buxton whether that difficulty was obviated when giving ether to little children.

Mr. Willett spoke of shock and mentioned the symptoms—pallor, dilated pupils, failure of the pulse, and so on. Perhaps it had not been mentioned that evening, because it was so well known, but was not it a fact that those were the symptoms which preceded vomiting? Several times he had been very much alarmed by such symptoms, and then the child was relieved by emptying the stomach.

DR. DUDLEY BUXTON, in answer to Mr. Grant Morris, said there was not the slightest difficulty

in giving the ether; the child went absolutely under, and the anæsthetist was never in the slightest doubt. Under chloroform the patient went to sleep, and it was difficult to distinguish the sleep from the chloroform narcosis.

The PRESIDENT said he was very much in accord with the late Mr. Mills's dictum, namely, not to give ether to patients under six or over sixty years of age. Dr. Dudley Buxton's very logical and scientific argument was one of the very greatest possible interest, but in spite of it, he (Dr. Silk) was afraid he remained quite unconvinced from the point of view of practice. The question of shock was a most important one. To some extent, however, he was rather sorry it had been introduced that evening, because it was so important, and of such a complicated nature that it was almost a pity it had been made a side-issue in a discussion on the administration of anæsthetics to children. Shock, what was really meant by the term, what was its physiological explanation, how to avoid it, how to counteract it when it did occur, were all questions worthy of quite a long discussion, which he hoped they might have subsequently. Personally, he confessed, he did not feel in a position to say much about it. As far as his experience went it certainly did not depend entirely upon light anæsthesia. Some people objected to the term shock, but whatever it was, he had seen quite as bad cases of shock when the patients were thoroughly under the anæsthetic as when they were lightly under. It was very interesting to hear such a good account of the mixture proposed by Mr. Willett, and he thought it was worth trying. With regard to the reason given by Mr. Willett for administering chloroform to children, some of the speakers had fallen foul of him for using the term fatty degeneration of the heart. Dr. Silk's view of Mr. Willett's argument was that *if* fatty degeneration was *ever* a factor which need be taken into account in the choice of the anæsthetic, at any rate such degeneration never occurred in children. So far he sympathised with Mr. Willett, although, of course, he quite agreed with the view that fatty degeneration was a mere bugbear from the point of view of the anæsthetist. He (Dr. Silk) was much impressed by Dr. Fowler's remarks from the point of view of a practitioner who had been in the

habit of giving considerable quantities of anæsthetic to many patients.

Mr. WILLETT, in replying, said he thought the subject he had introduced had resulted in a good discussion, as proved by the fact that nearly everybody in the room had spoken. He knew it was not an exhaustive paper. Two, at least, of the members had used the mixture he ventured to advocate, and both had spoken very highly of it, which was sufficient reason for inducing others to try it, including Dr. Fowler. As to shock, he thought Mr. Crouch rather misunderstood him. There must be at least two kinds of shock, and he understood Mr. Crouch that three patients died. He did not gather that they died at the time of the operation, but subsequently, so that it was quite possible the anæsthetic had nothing to do with the death. The shock that he (Mr. Willett) referred to was the sudden shock which occurred during a comparatively slight operation, and he wanted to bring out in the discussion whether the sudden shock occurred from the fact that the patient was not sufficiently under the influence of the anæsthetic. Some of the speakers had agreed with him, and others had not. Mr. Carter Braine had raised a question which it was difficult to answer, namely, when was a patient under? It was very difficult to tell, and insensibility on pinching the skin, he believed, was as good a test as any other. One advantage of the mixture he had advocated over chloroform was that with pure chloroform children were very apt to go to sleep, which made them appear to be quite ready for operation; the sleep induced by chloroform was a very different thing from the narcosis induced by the mixture. The advantage of the ether in the chloroform seemed to be to stimulate the breathing, and to prevent the patients from dropping off to sleep. Dr. Fowler had referred to a new kind of ether. Probably he meant Salamon's ether, which was the one he had used for the last three or four years. He much preferred it to Macfarlane's, as it was less pungent. Mr. Crouch mentioned tricks for getting patients under and gaining their confidence. There was a little trick which he (Mr. Willett) had used for some time, but which perhaps was hardly worth mentioning. In the case of children he often found that the mothers or nurses had been telling the patient to breathe

and smell the scent. But if the patient was asked "to blow it away," he would begin to blow furiously, and, of course, each time before he blew he took a deep inspiration, and the object was attained. A child would much prefer to blow than to take a deep inspiration. One of the speakers asked what should be done if deeper anæsthesia was required. In such a case, if he had his doubts whether the patient was sufficiently under with the mixture only, he used another bottle containing pure chloroform, a little of which could be sprinkled on the lint, which would overcome any difficulty. For ether he used the Clover's inhaler, but even that had a frightening effect upon children. Regarding the regulations at the London Hospital, Mr. Treves had told him that the reason they were made was because there were so many deaths from chloroform. He ventured to think that if the mixture he (Mr. Willett) advocated had been used instead of pure chloroform there would have been fewer deaths. With regard to fatty heart, he thought the President had given him credit for rather more ingenuity than he really possessed. At any rate the answer he gave students on the point made them think, and they did not question him again on that subject. He thought the President would agree that fatty heart did not occur in children. Whether fatty hearts on the post-mortem table were more frequently associated with chloroform administration than with ether he was not prepared to say. It was seldom that patients with fatty hearts died from the administration of chloroform.

Mr. WHITE asked Mr. Willett, in regard to the cases which were under the influence of the chloroform and ether mixture, which anæsthetic the patient was really under the influence of.

Mr. WILLETT replied that the patient was under a combination of the two. From his experience a mixture of the two, ether and chloroform, was better than ether alone.

BLEEDING from the surface of an internal organ or from a single vessel may be checked by directing against the seat of hæmorrhage a current of hot air (39° at 5 mm.) from the Holländer apparatus.—*Medical Record*, September 1st, 1900.

CORRESPONDENCE.

To the Editor of the 'Clinical Journal.'

SIR,

MAL DE MER AND ALLIED NEUROSES.

When an eminent authority makes such a statement as "we doubt if anything can prevent sea-sickness in certain persons and in certain conditions," it must appear great presumption on my part, to attempt to write anything on the subject. I am extremely liable to sea-sickness, and can thoroughly sympathise with all such sufferers, hence I have thought that perhaps the following observations might be of some interest to a very large and inclusive class.

A very simple experiment, which requires no elaborate apparatus for its performance, nor even an assistant, consists in placing two well-defined objects, at a distance respectively of three and six yards away from where the experimenter is sitting. He should then proceed to look rapidly from one object to the other for a considerable number of times, each time definitely sighting the object. This continual change of focus after a very short time, I found produced in me a feeling of giddiness, or more nearly a feeling similar to what I sometimes experience after a railway journey. From this I inferred, that possibly the so-called "railway sickness," was due to the rapidly changing focus occasioned by the eye closely following the apparent approach and withdrawal of objects such as hedges, railings, etc., at the side of the railway. As two eyes are necessary for properly focussing objects, and estimating distance, it occurred to me that, possibly by closing or covering one eye, and so interfering with my power of focussing, I might get rid of the irritating effects of railway travelling. Bearing this idea in mind, I resorted for a large number of railway journeys to this simple expedient, and I found that, whether sitting facing the engine or otherwise, I never suffered at all from the usual unpleasant effects.

Having personally tested this simple expedient several times, and with satisfactory results, I determined to find out whether the same treatment might be successful with other people, who suffered from railway sickness. The first person, whom I asked to try it, was a lady who had suffered

very considerably on the way down from Paddington to Swansea, so much so that she positively dreaded the return journey. She promised to adopt my suggestion, and to let me know the result, which was to the effect, that she reached Paddington without any bad symptom. Since then several people have resorted to the expedient of *occasionally* closing one eye when travelling, and with equally beneficial results.

As the result of experience, I find it is necessary only to occasionally close or cover the eye, these short intervals of rest being sufficient to prevent the nervous irritation, which leads to headache or sickness.

The similarity between railway and sea-sickness, suggested to me that possibly the same line of treatment might be successful in the latter. Being such a wretched sailor I hesitated, even for the sake of science, before taking a sea trip merely to test the applicability of my theory. Having occasion however this summer to cross from Swansea to Ilfracombe, with a friend, who dreaded the cross-Channel passage as much as I did myself, I asked him to join me in carrying out my suggested line of treatment. With this suggestion he was only too ready to fall in, and with the pleasing result, that although we had one or two small reminders of our poor seamanship, covering one eye restored us instantly to a peaceful state of mind.

On previous occasions I had been generally the first to suffer from sea-sickness, but this time I saw several on the steamer pay the usual penalty, without the necessity occurring of my doing the same. The next time I crossed I ventured to move about, and in so doing I came across a few persons who were showing a tendency to exhibit the usual symptoms, but who, on adopting my suggestion, were able to complete the passage in comfort.

In railway travelling, the continual change of focus is caused by the apparent approach and withdrawal of rapidly moving objects; on sea, this focal change is caused by our continually watching the top of some wave, and as soon as it is passed, rapidly gazing back on another wave top at about the same distance as the other began. To this process must be added the movements of the ship, which considerably complicate the mechanism of focussing a wave top, introducing as it were into main curve of focus-change, one, two or several *er* curves, and it is this complication or inter-

section of focus-curves, that causes the great nervous irritation resulting in sea-sickness. Covering one eye does away with all these curves, and their consequent nervous irritation. Covering or closing both eyes has not the same effect, for the simple reason, that when our eyes are closed, our imagination is sufficiently stimulated as to produce, by descending cerebral sensations, exactly the effects as were produced by external stimuli. Lying down *on one's back* is of considerable service in subduing sea-sickness, for the simple reason that one's eyes are focussed on the ceiling, and at a fixed distance. Lying on one side is not so efficacious, as one is able to see more, and within that field of vision there is sure to be some cause for continually changing the focus.

Closely associated with railway and sea-sickness, is the unpleasant fascination that one experiences when standing at the edge of a steep and dangerous cliff. What exactly causes this fascination, I am not in a position to say, save that I think in addition to external stimuli, we also get the result of cerebral sensations, the combination producing the unpleasant effect. To obviate it, one has only to close one eye, and he can remain at the cliff's edge as long as he likes, though, of course, the value of the view is diminished, as the perspective is destroyed.

G. ARBOUR STEPHENS, M.D.

Swansea.

Some of the Causes of Pains in the Feet.

—Hamann ('Cleveland Medical Gazette,' May, 1900) states that obscure pains in the feet may be classified into those due to acute, subacute, or chronic articular rheumatism, to gout, to gonorrheal rheumatism. The pains of gonorrheal rheumatism are usually persistent and difficult to cure. It may be divided into the monarticular and polyarticular forms, and classified into four groups: (1) hydrops articulari; (2) serofibrinous inflammation; (3) emphysema of the joint; (4) phlegmonous inflammation. Pain upon pressure over the insertion of the tendo Achillis is usually due to inflammation of the bursa retrocalcanea, which may be the result of any of the usual causes of inflammation.—*The Therapeutic Gazette*, August, 1900.

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*Specially reported for the Clinical Journal. Revised by the Author.

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A CLINICAL LECTURE

ON THE

MURMURS OF ANÆMIA.

Delivered at the Hospital for Consumption, etc., Brompton,

By ROBERT MAGUIRE, M.D., F.R.C.P.,
Physician to the Hospital, and to Out-patients, St. Mary's Hospital.

GENTLEMEN,—Our subject for this afternoon is the murmurs of anæmia. I have chosen it firstly, because these murmurs are common, and next, because their pathology is extremely interesting, and thirdly, because they are liable to be mistaken for murmurs of organic origin and therefore their diagnosis and prognosis is of importance.

Most of such murmurs are met with in chlorosis and are best studied in that affection, so my remarks will be mainly founded on observations upon that disease.

Let us consider the murmurs in question under the following heads. Those which are heard:

- (1) Over the veins.
- (2) Over the origin of the pulmonary artery.
- (3) Over the origin of the aorta.
- (4) Over the systemic arteries.
- (5) At the apex of the heart.

I want you to take particular note of this order. It is, I think, of importance in estimating the degree of the disease you have before you, and is also helpful in the prognosis of the individual case.

First I will describe these murmurs and their characteristics, and afterwards discuss their pathology. I begin with the murmurs heard over the veins, especially that one which is heard at the root of the neck immediately over the termination of the internal jugular vein. This murmur was first described by Laennec, and was thought by him to have its origin in the arteries; his description of the first time he heard these murmurs is given in the original edition of his work on Auscultation.

I may premise that these murmurs are obviously connected with the circulation of the blood through the veins; as a rule they are continuous, they are also extremely musical; further, the tone produced in them is very variable, and this is shown in the account Laennec gives. He says that when examining a lady's chest for suspected phthisis he heard a peculiar sound which he thought must proceed from some one playing an instrument in the next apartment, but at last satisfied himself that the sound was produced in the body of the patient herself. He has placed in his book the musical notation of the murmur.



On his next page he makes a note of the tonicity of two other such murmurs. Once I noted the musical sequence of such a murmur, but of much more varied tonicity, and heard over the chest of a patient in St. Mary's Hospital. Unfortunately the notes of the case have been lost, and I cannot record them here. The murmur is more musical than any other you can hear in connection with the circulatory apparatus. As a rule it is best heard on the right side of the neck; sometimes it is not heard on the right side, but heard well on the left, the explanation of this not being obvious. It is louder, or sometimes only heard, in the upright position, because the rate of the flow of blood to the heart is increased in this position. Because of this fact, never examine for these murmurs when the patient is lying down in bed; the flow of blood is not then so rapid, and the head being bent forward from the pillow the vein is further from the stethoscope and the murmur may be missed. For the same reason the murmur is louder during inspiration, and also louder during the diastole of the heart than during the systole; consequently, when inspiration and diastole of the heart happen to coincide the murmur is at its maximum of intensity, and again, when expiration and systole coincide, the murmur may be altogether stopped for a time.

These variations may possibly trouble you at times, for the murmur may be only present during the diastole of the heart. Now it is sometimes

not confined to the root of the neck, but heard over the manubrium sterni, and if you hear a diastolic murmur in that neighbourhood you ordinarily expect to find aortic regurgitation as the cause of it, whereas the real state of things may be chlorosis with its accompanying conditions. If you are in doubt about the character of such a diastolic murmur you must examine for the conditions which accompany aortic regurgitation, most of which are absent in chlorosis. A diastolic murmur due to chlorosis is not so regular as the diastolic murmur of aortic disease, and it is not carried down to the xiphoid cartilage, moreover it usually ceases when the patient holds her breath. Certain other positions also increase the intensity of the murmur, and sometimes are the only means of at all bringing the murmur into evidence. The most important of these is the turning of the head to one or other side; the murmur is heard in pronounced cases when the face is looking forward, but it is nearly always increased in intensity by turning the head to the opposite side. Do not, however, let the patient turn her head herself, for a mere twisting of the chin will not produce the effect required. Observe, too, that while turning of the head to the opposite side will increase the murmur, yet if the head be turned so far as to entirely straighten the neck the murmur may be stopped entirely. It is said by some writers that mere turning of the head to one side will produce such a murmur in the absence of anæmia, but my own observations do not confirm this. I have never heard the murmur in any position unless anæmia were present.

The next murmur I wish to describe to you is one heard at the origin of the pulmonary artery. It is systolic in rhythm, always blowing in character, very soft, and not interfering with the sounds of the heart at that particular spot. It is heard best at the junction of the third left costal cartilage with the sternum, and is carried upwards and towards the left, very rarely downwards. It is best heard, as a rule, when the patient is lying down, thus differing from the venous murmur already described. Further, this murmur is best heard during expiration, again differing from the venous murmur, and I may anticipate what I wish to say about its pathology by remarking that this phenomenon is caused by

the fact that during inspiration the lung expands, lies more thickly over the pulmonary artery, and therefore a murmur produced in this neighbourhood will be more or less hidden from the ear during inspiration.

There is also to be observed a murmur over the aortic cartilage similar to the pulmonary murmur. It is systolic in rhythm, and is never so loud as the pulmonary murmur; it, again, is blowing in character, and does not interfere with the sounds of the heart. It is heard best at the junction of the second right costal cartilage with the sternum and, in intensity, is carried upwards and towards the right.

Certain functional murmurs heard in the systemic arteries are similar to those heard at the root of the pulmonary artery and aorta, but such murmurs due to anæmia are only heard in the large arteries, over the subclavian especially, where it appears beneath the outer end of the clavicle; you can hear the same murmurs in the carotids and in the femoral arteries, and you may also include in this series the murmur which is frequently heard over the abdominal aorta from a little below the epigastrium to the bifurcation of the vessel. It is important to recognise these murmurs as anæmic in origin because they are liable to be mistaken for the murmurs of an aneurysm, especially, too, when, as in chlorosis, the arteries, particularly under the influence of some slight excitement, seem to be much more pulsatile than normal. I have often found such a mistaken diagnosis occur. In the case of the murmur in the abdominal aorta which I have mentioned, the patients are frequently dyspeptic and complain of stomach pain, the examination leads to the discovery of extensive pulsation of the abdominal aorta, the stethoscope reveals a murmur, and so, without care, the diagnosis of aneurysm is made. In a paper recently published and entitled "Aphorisms" I said, "Whenever you find pulsation in the abdomen say there is not an aneurysm." The foregoing observation was the basis of this remark.

In speaking of the venous hum as it is sometimes found over the manubrium sterni I mentioned that it is sometimes diastolic in rhythm. Here, you would say, is one of the physical signs of aortic regurgitation, and if the sign be combined—as it often is—with excessive beat of

the vessels a wrong diagnosis may easily be made. Remember this and be warned accordingly.

These, then, are the vascular murmurs which are heard in anæmia, and their pathology, though yet uncertain, is extremely interesting. In discussing it we must first consider especially the intrinsic condition of the blood, that is particularly: its specific gravity; secondly, the quantity or mass of the blood; and thirdly, its tension, all of which are important.

The murmurs under discussion have been called hæmic, the supposition being that they were produced by the fewness of the blood-corpuscles in anæmia. Like many of the theories which have been carried into the text-books this has had no foundation in fact. Although it might be surmised, of course, that a limpid fluid might be thrown into quicker vibration than a thick fluid, and although it was thought by some distinguished physicians that such a state of blood might exist, yet there was no proof of this till recently. Potain experimented by passing the serum of blood, and again blood containing corpuscles, through a series of tubes, the tubes being arranged according to the anatomical condition of the jugular vein. A murmur was found to occur when corpuscles were contained in the fluid, and was absent or diminished when the corpuscles were not present. This is the only evidence we have, so far as I know, of the physical condition of the blood causing or preventing a murmur during the blood's passage. If the condition of the blood were the cause of such a murmur, we might roughly imitate its production in the following way. Tightly fill a tin box with peas. Shake it, and no noise occurs. But if you fill it half full a noise is produced from the irregular movement of the peas, which represent the corpuscles of the blood. Take the next most obvious change of the blood, the alteration of its specific gravity. Of this we have only recently learnt anything at all, and our knowledge is especially gained from the observations of Sherrington and Copeman. They have found that in chlorosis the specific gravity of the blood is considerably diminished; in chlorosis it may range from 1038 to 1041; in pernicious anæmia they found it is from 1028 to 1035; but in lead poisoning where there is often a grave amount of anæmia, the specific gravity is almost the lowest they found in any

disease, namely, 1031. I think it certain, from my own observations, that the murmurs I have been speaking about are much more common in chlorosis than in any other form of anæmia, not excepting pernicious anæmia. All the murmurs may be present in these anæmias, but they are not, as a rule, present in lead poisoning, therefore it is obvious that the specific gravity cannot be the cause of the production of anæmic murmurs, since the specific gravity is lowest in lead-poisoning, and yet murmurs are not usually present.

Further, you will find the venous hum present in the very slight anæmia which accompanies recovery from acute rheumatism. Take any of the cases of acute rheumatism in the hospital just at their recovery, and listen over the termination of the jugular vein when the patient is sitting up, and you will hear this venous hum. There is in this state a certain amount of anæmia, and naturally there is also diminution of the specific gravity of the blood. But as we have shown that these conditions exist elsewhere without the production of murmur they cannot possibly be the only cause of such murmurs.

Next, I came to the mass of the blood. We can refer to one observation in this matter, and, as far as I know, only one, that by Quincke. He found that there was a diminution of the whole volume of the blood in cases of chronic anæmia. You may have anæmia without such a diminution, for twenty ounces of blood abstracted from a person is soon made up in volume from the tissues of the body. In chlorosis there is no possibility of determining what the mass of the blood may be, although the probability is that the mass is diminished. There is also an observation of Clifford Allbutt's which, with all deference, I think is founded on an unsound basis, for he says that because the arteries often beat so strongly in chlorosis, the probability is that the amount of blood is increased; yet the arteries may beat strongly and the mass of blood be diminished.

Next, let us consider the tension under which the blood is contained in the vessels. Undoubtedly the vessel walls do aid to some extent in the conduction of the murmurs under discussion, for you can feel the vibration of the vessel walls with the fingers. Place your thumb very lightly over the region of the jugular vein in this patient and feel a distinct thrill corresponding in rhythm

with the hum heard by the stethoscope. On the other hand, place your stethoscope on the vein and press very lightly (not enough to stop the course of the blood through the vein) and the murmur goes on just as before; obviously the vibration of the vessel wall is a mere accident produced by vibrations in the current itself.

Now if the mass of the blood were diminished you would expect the blood to be under less than normal tension. But the vessels have an extreme power of adapting themselves to the volume of blood supplied to them,—they can contract down on the blood.

It has been said by good authority, by what I think the best authority, that the arterial tension in anæmia, as shown by an observation of the radial pulse at the wrist, is always increased; but in spite of this authority, I am bound to state that I myself cannot detect such increased tension as a universal condition in anæmia. There is, I think, no question whatever, that the writers who have described this increased pulse tension felt it, but I have attempted to get some more direct evidence of it by means of the sphygmometer. Perhaps from inexperience with the instrument I have had no reliable results.

But I made an experiment myself the other day which throws light on these observations. A case of anæmia came into the out-patient room at St. Mary's Hospital, and I asked eight of the students to examine the pulse tension,—these students were men whom from past experience I could rely upon more than one usually can rely upon students for an examination of the pulse, and also I myself examined it. I asked them all to write down their opinions. Most of the opinions differed, and they all differed from mine; upon another examination a short time afterwards my own opinion altered. It is well known that all neurotic patients have a pulse which, to say the least, closely resembles that of high arterial tension; it is full between the beats, and pressure is required to overcome its tension. The pulse of high tension found in neurotic patients is sudden, and sustained for but a short time, while the artery remains full between the beats. The tension is probably produced by a neurotic contraction of the muscular coats of the smaller arteries. The condition of things is the same in chlorosis, for the neurotic

girl, in spite of her general apathy, is very easily excited, and especially so when being medically examined. But if the patient be examined quietly when her attention is otherwise engaged you will find that the pulse is not one of arterial tension, and my opinion is that its customary condition is one of low arterial tension.

The fact is, I think, that in chlorosis we have a combination of all these three conditions which may produce murmurs, viz., a diminution in the mass; probably a diminution of the specific gravity of the blood; and the arteries and the veins, which upon the diminished mass of blood ought to contract, do not. In lead poisoning you have the two first conditions but not the third, for there is here true high vascular tension, consequently there is no murmur. But in other disorders where, as in acute rheumatism, there is a large full artery which drops suddenly after the systole, there is the requisite laxity of the vessel wall, and the murmur is found. The pathology of these murmurs is then probably a combination of a diminished mass of the blood, a diminished specific gravity, and a lack of tension of the vessel walls, whether venous or arterial.

This view of the pathology of such murmurs gives a reason for some of their conditions. First, as regards the presence of the venous murmur heard in the neck. The jugular vein passing down the neck joins on the right side the subclavian vein to form the right innominate, and at the junction of the two there is a small dilatation, sometimes called the sinus jugularis. This is embedded in fibrous tissue distinctly stronger than that which envelops the vein in the upper part of its course. When the mass of blood is diminished in amount of course there will be an attempt on the part of the vessel wall to contract, and this might easily be accomplished in the upper part of the vein, but not so readily in the lower part surrounded by the firm fibrous tissue, consequently there will be a flow of blood from a narrow to a wider part, the condition most likely to produce *veines fluides*, the currents which cause murmurs. Again, when we turn the head to one side we tighten the tissues and so render still narrower the vein in the neck, thus increasing the difference of calibres, and increasing the intensity of the murmur. But if the head is turned still more to the side, either the blood-

flow is stopped entirely, or the fascia is so tightened that the sinus is reduced to the same calibre as that of the vein above. In either case the murmur disappears. The left innominate vein joins the right innominate, and the flow of blood from the left jugular will not be so rapid as that in the right, because the direction of the left innominate is more at right angles than that of the right. Further, the left jugular vein is situated under the sterno-mastoid more than the right is, and is not so liable to be affected by movement or to be pressed upon by the omohyoid muscle. Thus we have an explanation of the greater frequency of the murmur on the right side.

I need only mention here two other views which have been put forth of the systolic murmur heard over the root of the pulmonary artery, one that this murmur is due to regurgitation through the valve, and the other that it is caused by pressure of an enlarged left auricle on the vessel; of both conditions there is no evidence. The explanation I have put forward explains the order of frequency of the murmurs. The first murmur to be heard in chlorosis is the venous hum, next the pulmonary, then the aortic, then the systemic murmurs. Why should this be? First, because the tension in the veins is lower than elsewhere, and the walls of the veins are less likely than the other vessels to contract down upon the diminished mass of the blood.

Next, the pulmonary artery has thinner walls than the aorta, and therefore you would expect it to, more easily than the aorta, show the conditions necessary for the production of the murmur. Further, the pulmonary artery and aorta contain hardly any, if any, muscular tissue at all, therefore they are less likely to contract upon the mass of blood, and this explains, I think, why the murmurs in the systemic arteries are the least frequent of all. In them the muscular tissue is better able to fit the calibre of the vessel to its contents than is that of any other part of the vascular system.

The murmurs heard near the apex of the heart are, I believe, of two kinds. Both are systolic in rhythm, but one (1) is heard best slightly inside the apex of the heart, is conducted best in the upward direction towards the pulmonary cartilage, and is not accompanied by any sign whatever of

cardiac dilatation. It is not conducted to the back, it does not interfere in any way with the first ventricular sound as heard at the apex, the pulmonary second sound is not accentuated and is not re-duplicated, or does not appear to be re-duplicated, wherever it may be examined. The other (2) is heard best exactly at the cardiac apex, is conducted to the left and through the axillary region so as to be well heard in the back near the angle of the scapula, and is accompanied by definite signs of left ventricular dilatation. Moreover, with such murmur there is, often, and, indeed, usually, accentuation of the pulmonary second sound, and often re-duplication of that sound in some part or other of the cardiac area.

In passing, let me say a word about the accentuation of the pulmonary second sound mentioned above.

I find students and some practitioners seem to have an idea that because the tension of the blood in the aorta is greater than that of the pulmonary artery the second sound ought to be louder over the aorta than over the pulmonary artery. But this is not so. The second sound is better heard at the pulmonary area because the pulmonary artery lies in front of the aorta, and its second sound will be the more easily conducted through the tissues. Vierordt determined this accurately by measuring the intensity of the two sounds. He inserted discs of india-rubber as buffers between the stethoscope and the chest wall, estimating how many were required to annul the aortic and pulmonary second sounds respectively. He found that the pulmonary sound was the more intense. Be careful, therefore, not to mistake the physiological condition for accentuation of the pulmonary second sound.

The second of these murmurs is undoubtedly caused by mitral regurgitation, the result of a relaxation of the muscular tissue of the left ventricle. As you know, the mitral valve, for its full functional activity, must be assisted by the muscle of the ventricle, and if that muscle is weak regurgitation through the mitral valve results. Thus are caused the murmur and its accompaniments, which are simply those of mitral regurgitation as seen elsewhere.

The first murmur described has had the same pathology ascribed to it and, I think, wrongly. I have particularly mentioned how with this

murmur no sign whatever of cardiac dilatation can be found. We are wrong to assume the existence of such dilatation without evidence, and yet that is what has been done. We may leave the pathology of the second murmur without further discussion. It is due to cardiac dilatation, mitral regurgitation, and the whole caused by a condition—muscular relaxation—which has been added to the original anæmia. But as to the second murmur. Does it not come into the same category as all the murmurs mentioned in the list at the beginning of this lecture? Remember too, that when one is obliged to make two diagnoses in the one patient there is always a likelihood of error. So, too, if two clinical states, occurring under the same conditions, have to be assigned to two different pathologies, there is a probability of error.

Now this murmur, which is unaccompanied by any sign of cardiac dilatation, surely ought to be included with the vascular murmurs, and I would ascribe to it the same pathology. It is due, I believe, to lack of tension of the blood-containing walls, causing, as it were, a churning of the blood and consequent fluid whorls, the effect possibly being assisted, as in the former instance, by an alteration in the condition and mass of the blood.

Thus I would place all the murmurs found in anæmia, with one exception, in the same pathological class, the exception being that in which muscular relaxation of the heart is added to the ordinary pathological conditions of anæmia.

The pathology of the murmurs is of importance in judging the prognosis of the case. The amount of the anæmia may be judged by counting the red blood-corpuscles, but this will not give a true idea of the whole of the patient's condition. There may be only a slight degree of corpuscular deficiency and yet a considerable amount of laxity of tissue, while both conditions are evidenced by the presence of the murmurs, and an estimate of the amount of the general disorder may be obtained from the detection of the murmurs in the order above described.

CYCLIC ALBUMINURIA.

BY

G. A. SUTHERLAND, M.D.,

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THE term cyclic albuminuria was introduced by Dr. Pavy in 1885 as a distinguishing name for a form of albuminuria which had previously been referred to by various writers as "albuminuria in the apparently healthy," "physiological albuminuria," "intermittent albuminuria," and "albuminuria of adolescence." He described the characteristic features of this condition in the following terms:—"In the early morning the urine is free from albumin. Albumin then shows itself—it may be at 9, 10, or 11 a.m., or not till the early part of the afternoon. After reaching its maximum it declines, and often by the evening it has disappeared. It is rare to find that it has not disappeared by bedtime." In a further communication in 1886 he described a number of tests which he had employed to determine the active factor in producing this diurnal cycle, and concluded that it was the change of position on rising from bed. In all of his cases the albuminuria was accidentally discovered, and no other symptoms of ill-health were complained of, or observed. Since these papers were published much attention has been directed to this subject, more especially on the Continent, where the affection is referred to as Pavy's disease (*Die Pavy'sche Krankheit*, *Maladie de Pavy*); but in this country comparatively few cases have been recorded. In addition to the names given above, various alternative titles have been used, such as "postural albuminuria," "albuminurie orthostatique," and "albuminurie de la station debout" (Teissier), "orthotische albuminurie" (Heubner), "renal migraine" (Marie), and "neurotic albuminuria."

Dr. Pavy did not assert, nor will it be asserted in the following pages, that cyclic albuminuria is the only form of functional albuminuria—that is to say, albuminuria not associated with organic disease of the kidneys. There are the passing conditions

of pyrexial albuminuria, of exertion or fatigue albuminuria, and there are certain individual peculiarities which lead to albuminuria after special articles of food. There are also mere traces of albumin met with in some individuals, regarding the significance of which we know nothing. But my own experience has been that cyclic albuminuria is by far the most common form of functional albuminuria met with in adolescence and early adult life.

The literature of the subject shows a diversity of opinion about most of the problems still surrounding the subject of cyclic albuminuria, which I do not propose to give in detail here, but shall discuss later on. It is probable that different authors have been describing cases of different kinds, since a cyclic element as regards the occurrence of albuminuria has been found to occur apart from the condition of pure cyclic albuminuria,—for example, in typical Bright's disease. This has led to a certain amount of confusion, and renders the compilation of correct statistics a matter of great difficulty, so that for purposes of clearness it might be as well to use Heubner's term, "pure cyclic albuminuria," for the class of cases under discussion. In addition to the papers by Pavy already referred to, valuable articles on the subject of cyclic albuminuria have been published by Heubner, Marie, and Teissier; and, speaking generally, the views of these authors are in agreement. The description of the affection to be given in the following pages is based on a study of fifteen cases which could be thoroughly tested, and on others which were less completely observed in outpatient practice. As the examination of a case entailed the analysis of every specimen of urine passed, which would amount to from one hundred to four hundred analyses in a single case, the work of my house physicians was not light; and, carried out as it was in the most ungrudging spirit, my debt of gratitude to each of them is a heavy one.

SYMPTOMS.

The characteristics of this form of albuminuria are that the urine passed in the morning, before or immediately after rising, contains no albumin; that after the patient has been up and going about for some time, the next specimen passed contains a definite amount of albumin; and that the albumin

diminishes in amount as the day goes on, and is absent or present only as a slight trace when he retires to rest. This is the normal cycle of the albuminuria under the normal conditions of daily life. For purposes of clearness this variety of albuminuria may be described as cyclic in character, postural in origin, and adolescent as regards the period of life when it most frequently occurs.

If the patient is kept lying down in bed the albumin will remain absent from the urine during the time he is at rest. Most of my patients have been kept in bed for some days with this result, and within a few hours of their rising again albumin has reappeared in the urine. Others have tested this for much longer periods with similar results. Rooke kept a patient lying down and under observation for three months, and during that time there was no albuminuria; but the albumin reappeared as soon as he got up. Heubner refers to a similar test, carried on for nine months, with a similar result. Sitting up in bed may possibly lead to a slight amount of albuminuria, but not nearly as much as after adopting the erect posture. In my own experience this has occurred very exceptionally. Passive exercises or active movements while the patient is recumbent do not induce albuminuria, nor is it brought on by changes of diet or by any special articles of diet. As long as he maintains the recumbent position this condition of albuminuria cannot be artificially produced. When he is up and going about the converse holds true—namely, that the occurrence of albuminuria cannot be artificially prevented. The condition is not the result of taking food, because it will appear before any food has been taken if the patient has been going about meanwhile. It is not dependent on, although probably increased by, walking exercise. By the addition of water to the diet one can diminish the proportion of albumin by increasing the amount of urine passed; but this does not influence the incidence of the albuminuria, or in all probability the amount of albumin passed per diem. The period of the day is of no importance, because, if the patient rises in the evening, the albumin will appear as regularly as if he rose in the morning.

The term cyclic is applicable only in the sense that, under the normal conditions of life, rising in the morning and the appearance of albumin in the urine form part of the daily cycle in these patients.

is shown in the following table:

TABLE I.—*Showing the condition of cyclic albuminuria under the ordinary conditions of life.*

	Urine	...	5 a.m.	...	Albumin. 0 per cent.
Rose 7 a.m.	"	...	7 "	...	0 "
	"	...	9 "	...	'35 "
	"	...	12.45 p.m.	...	'15 "
	"	...	4.45 "	...	'1 "
	"	...	5.30 "	...	Trace.
Bed 6 p.m.	"	...	6 "	...	"
	"	...	5.30 a.m.	...	0 per cent.

This cycle can be made a double one simply by putting the patient to bed for a few hours during the day. The period of rest is accompanied by a disappearance of the albuminuria, and is followed by its reappearance, as shown in this table:

TABLE II.—*Showing a double cycle of albuminuria within twenty-four hours, dependent on change of posture.*

	Urine	...	3.45 a.m.	...	Albumin. 0 per cent.
Rose 4 a.m.	"	...	5 "	...	'5 "
	"	...	6 "	...	'2 "
Bed 6 a.m.	"	...	6.45 "	...	Faintest trace.
	"	...	1 p.m.	...	0 per cent.
	"	...	1.30 "	...	0 "
	"	...	2 "	...	0 "
Rose 4 p.m.	"	...	4.45 "	...	'1 "
	"	...	5.30 "	...	'3 "
Bed 6 "	"	...	6.5 "	...	'1 "
	"	...	5 a.m.	...	0 "

The following is a summary of the observations kindly made for me by Dr. Cecil Hayward during a period of fourteen successive days in the case of a girl twelve years old.

Day 1.—Admitted 10 a.m. and passed urine which contained albumin. Went to bed at once, and no more albumin passed that day.

Day 2.—In bed all day, no albumin in any specimen.

Day 3.—Up for two hours in the morning before any food taken, and the urine passed at the end of this time contained albumin ('25 per cent.), which again disappeared soon after patient went to bed.

Day 4.—In bed all day, no albumin in any specimen.

Day 5.—Got up for two periods, morning and evening, of two hours each. Result—a double cycle of albuminuria, albumin appearing after each rising.

Day 6.—Up all day. The specimen passed on rising was free from albumin; all the others passed during the day contained albumin.

Day 7.—In bed all day, no albumin in any specimen.

Day 8.—Up for two hours (7—9 a.m.). No albumin save in specimen passed at 1 p.m.

Day 9.—Rose at 3 p.m. One specimen previously at 1 p.m. contained a trace of albumin (? from sitting up in bed). After rising next urine passed contained albumin ('25 per cent.).

Days 10 and 11.—A repetition of day 5, with double cycle of albuminuria.

Day 12.—Up all day. Albumin in each of the five specimens of urine passed after rising.

Days 13 and 14.—Continuously in bed, no albumin in any specimen.

It is now generally recognised that, whatever other factors may be at work, the immediate cause of the appearance of albumin in the urine is the adoption of the erect posture. In some cases albumin has been detected in the urine fifteen minutes after the patient rose from bed, and has disappeared in about the same time after his lying down. Obviously this cannot be tested in most cases, as urine is not secreted freely enough. As a rule the albuminuria is well marked in a specimen passed two hours after rising; sometimes its appearance is delayed for four or five hours. As a rule, also, it has passed off entirely before the end of twelve hours, although in some cases it continues as long as the patient is up and about.

As regards the quantity of albumin present, it is sometimes stated that in "functional" cases only a small quantity or a mere trace is present. In my own cases this was not so. The albumin was considerable in amount as manifested by the ordinary tests, and such as to suggest at first sight serious renal disease. On boiling, the deposit would amount to one sixth, or one half, or even in some cases two thirds. On testing with Esbach's albuminometer it would amount to as much as '1 per cent. to '4 per cent., and in one case to 1'4 per cent. I do not mean that albumin to this amount was present every day, but simply to emphasise the fact that a considerable amount will usually be discovered when these patients seek advice, provided that the urine is examined at the proper time after rising. Keller found that the daily amount of albumin passed by a girl of

eight years with this affection was fifty-one centigrammes. Lacour found forty centigrammes per litre in another case. The fluctuations in the amount of albumin from day to day are very striking, and seem to depend on causes which are not yet properly understood. In individual specimens the proportion will of course vary with the quantity of urine passed. A period of good health is frequently accompanied by a diminished amount of albumin. Heubner has found that cyclic albuminuria sometimes passes off entirely for days or weeks, but this in my experience is very exceptional. Taking the records during in-patient treatment, when every specimen of urine passed was examined, I find it very unusual for a single day to pass without the occurrence of albuminuria if the patient was up and about. As regards out-patient records they are necessarily more irregular, and refer as a rule to examination of the urine passed on rising and about 11 a.m., at intervals varying from a week to a couple of months. Some of the patients have been under observation for several years, and I find that on very few occasions has albumin been absent from the forenoon urine, and that the note is almost always the same, namely, no albumin or a mere trace of albumin in the urine passed on rising, and a definite amount in the specimen passed at the hospital. While my observations agree with those of Heubner in that there are exacerbations of the affection, characterised by headache, lassitude, and an increased albuminuria, they differ in that I have not been able to determine the existence of periods in which the albuminuria ceases entirely under the normal conditions of life.

Pavy found that the albumin present in these cases was a mixture of alkali albumin and serum albumin. Dickinson has noted that the albumin is readily soluble in excess of nitric acid, an observation which has been frequently confirmed in my own cases. Teissier and others have found nucleo-albumin, and probably the same constituent is referred to by those writers who speak of an excessive amount of mucin. Nucleo-albumin is a substance precipitated from certain urines by the addition of acetic acid in the cold, and said to be soluble in excess of acid, but the excess must be very great. The origin and significance of nucleo-albumin are still undetermined. In my cases of cyclic albuminuria it was a constant accompani-

ment of serum albumin in the urine, and the greater the amount of serum-albumin the more marked was the nucleo-albumin reaction. After coagulating the albumin by boiling, and removing the deposit, the fluid left gave the nucleo-albumin reaction. It was only very rarely that the non-albuminous urine—that is to say, the urine of rest—was found to contain nucleo-albumin, and then only a trace could be detected. In cyclic albuminuria it would appear that nucleo-albumin is a regular accompaniment of serum albumin, the two appearing and disappearing under similar conditions, and also increasing and diminishing in amount at the same time.

The total amount of urine passed by these patients in twenty-four hours is usually small, and frequently it is very much below normal. This is probably partly dependent on the facts that they are liable to sweating, and that they do not as a rule drink much. Polyuria was never present in my cases as a symptom. It is not uncommon to find that as much urine is passed in the morning on rising as during the whole of the rest of the day, a state of matters which is the opposite of what occurs in health. In some cases, as may be seen in the following table, the amount of the night urine is very greatly in excess of that passed during the day.

TABLE III.

Case (a), showing large amount of urine passed after rest as compared with that passed during the day.

Hour.	Urine.	Hour.	Urine.	Hour.	Urine.
5 a.m.	17 oz.	5 a.m.	17 oz.	4 a.m.	17 oz.
9 "	5 "	4 p.m.	5 "	1 p.m.	5 "
1 p.m.	6½ "			4 "	3 "
7 "	6 "			6 "	2 "

Case (b), showing amount of night urine (thirteen hours in bed) and day urine (eleven hours out of bed) during seven consecutive days.

	Ounces.						
In bed	21	15	22	25	32	20	20
Out of bed	9	5	10	6	6	10	3

Case (c), as above.

	Ounces.						
In bed	22	23	26	20	19	22	22
Out of bed	16	9½	9	8	14	13	12

With the exception of the absence of albumin, the urine secreted during the night does not

appear to differ in composition from that secreted during the day. Even in those cases in which the amount of urine secreted is below normal, by the addition of water to the diet one can rapidly increase it to fifty or sixty ounces without affecting in any way the condition of albuminuria. This is shown in the following table :

TABLE IV.—*Showing increased amount of urine produced by taking fluid without affecting the incidence of the albuminuria.*

Hour.	Urine in ounces.	Albumin. Percentage.	Patient.
1 a.m.	8	0	In bed.
5.30 "	4	0	"
6.45 "	5	0	"
8.25 "	4½	0	"
10.35 "	4½	0	"
12.45 p.m.	2½	0	"
			(Rose 1 p.m.)
3 "	2½	.02	Out of bed.
5.40 "	4	Trace	"
7 "	1½	"	"
			(Bed 7 p.m.)
9 "	5	0	In bed.
10.15 "	5	0	"
11.30 "	5	0	"
12.35 a.m.	6	0	"

Total 57½ ounces.

Loaded urines, with a high specific gravity, are the rule in these cases. On cooling, a deposit of urates, uric acid, or oxalates quickly takes place. The passage of these substances does not seem to be accompanied by any special irritation as shown by the absence of frequency of micturition, or hæmaturia, or pain about the bladder. The urine is usually acid, often markedly so ; but in some cases it is neutral or alkaline, and phosphates may be present in excess. These conditions are in all probability partly due to the small quantity of urine passed, and are usually removed when a normal amount of urine is secreted.

As regards the presence of casts, in none of my cases were they found on examination by the ordinary methods, or after centrifuging. Heubner has also examined carefully for casts after centrifuging the urine, and has failed to find them. With reference to the statements of some writers that after centrifuging they were able to detect both casts and tubular epithelium, two criticisms may be made. First, that the same discovery has

been reported by some authorities in the case of non-albuminous urine passed by healthy individuals. Secondly, that in organic renal disease with an amount of urine such as is present in these pure cyclic cases, casts are found with great ease and in great abundance, even without the use of a centrifuge. If tube casts, other than hyaline, are found on repeated examination, either with or without centrifugalisation, I think that a diagnosis of pure cyclic albuminuria cannot be made, and that something further must be suspected of the nature of renal inflammation. Such suspicions are strengthened if there is a history of previous scarlet fever or diphtheria with renal complications. As regards the presence of blood or sugar in the urine, I have never met with either of these substances in connection with cyclic albuminuria, nor do I know of any other writer who has done so.

Attention has been concentrated chiefly on the condition of the urine in these patients, many of whom are described as "apparently healthy." The albuminuria has often been discovered accidentally, and no other symptoms have been complained of. A possible explanation of this will be given later in considering the progress of the affection. My own experience has been different, for almost all the patients sought medical advice on account of general ill-health. In the earlier years of this investigation the symptoms complained of were not recognised as sufficiently definite for the purpose of diagnosis, and it was only by going over the different systems *seriatim* that the albuminuria was discovered. More recently, with a fuller knowledge of the symptoms usually complained of, I have sometimes been able, from the patient's history, to suspect a condition of cyclic albuminuria, which was found on examination to be present. In other cases, although the history and the symptoms closely resemble those about to be described, no albumin was found in the urine. The general symptoms about to be described are most pronounced during early adolescence.

The subjects of this affection are nervous, excitable, and emotional. Sometimes they have been regarded as such all their lives, sometimes the condition has only been specially noted after an acute illness such as influenza. The onset of acute symptoms, from the parents' point of view, is usually between the ninth and twelfth years. A child who has hitherto presented no special

characteristics suddenly becomes touchy, weeping on the slightest provocation, or irritable, or passionate. This condition may be intermittent and transient, or may become persistent, and be accompanied by an appearance of chronic ill-health. Such children sleep badly at night, their rest being broken frequently by night terrors, talking in their sleep, or somnambulism. They are subject to attacks of lethargy, are easily tired and easily depressed, and suffer from prostrating headaches, which may be accompanied by vomiting, visual disturbances, etc. These headaches seem to be of the nature of migraine. Pain in the back and the limbs, and general aches such as suggest rheumatism, are very common, without any objective changes, or any accompanying cardiac complications. These patients sweat easily on emotion, or excitement, or exertion. While the general health remains below normal the course of the illness is characterised by remissions in which the symptoms moderate or pass off, and attacks in which they reappear acutely. These attacks may be described as nerve-storms, which at times cause disturbance in the central nervous system, and at other times in the peripheral systems only.

The alimentary system is the seat of various troubles. The appetite is variable, and a special liking often exists for meat and highly spiced foods, with a corresponding distaste for milk and puddings. Sudden attacks of vomiting with headache, fever, and signs of gastro-intestinal catarrh are frequent. Some of these attacks are traceable to the dietetic habits—excessive amount of meat, etc.,—while others are not. Sore throat (tonsillitis) with pains in the limbs is often complained of. Abdominal pain is another marked symptom, and may be so severe as to disturb the patient's sleep. This is usually referred to the region of the umbilicus, or to the epigastrium, and may in some instances be dependent on renal irritation from excess of uric acid or oxalates, but in others such an explanation does not seem to apply. These attacks from their suddenness of onset, their intensity, and their unexplained origin, may be described as gastric crises. In some cases the pain is referred to the right iliac region, and is such as to suggest appendicular inflammation. Constipation is usually present, and will frequently be found to depend on an atonic condition of the lower bowel, and to be associated with faecal accumulation in the caecum,

colon, or rectum. Alternating with constipation there may be attacks of diarrhoea.

The circulatory system is subject to various disturbances. A condition of anæmia may be regarded as one of the characteristics of the affection, but is apparently symptomatic rather than organic. A tendency to redness, or blueness and coldness of the extremities usually exists, and the patients are subject to flushing or fainting attacks, and to passing erythematous eruptions. The sudden appearance of a diffuse erythema with sore throat and slight pyrexia, may at first sight simulate an attack of scarlatina. Swelling about the eyes and the ankles is not uncommon. This is usually moderate in amount and transient in character, subsiding on rest. Sometimes, however, there is a persistent puffiness of the lower lids, which at first sight suggests renal disease. Dr. Theodore Fisher has specially noted the occurrence of swelling of the eyelids with intermittent albuminuria in children whom he describes as delicate, with a listless aspect, but without evidence of organic renal disease. Dark circles often appear round the eyes, and may be accompanied by pigmentation of the skin elsewhere. In one case the whole surface of the body had become of a dirty yellow colour. Epistaxis has occurred in several of my cases, and afforded considerable relief to co-existent headache. Marie has stated that hæmoptysis is a rare symptom. Examination of the heart shows that the cardiac condition is disturbed rather than diseased, and it varies considerably from time to time. There may be diffused and heaving præcordial pulsation. In two of my cases tachycardia was present, which usually subsided rapidly when the patients were kept in bed, but reappeared on rising. A persistent increase in the frequency of the cardiac pulsations, which reach from 90 to 100 per minute, is common, quite apart from any movement or excitement. There is no cardiac hypertrophy, but the first sound at the apex may be reduplicated, producing a cantering rhythm, or blurred, or followed by a short systolic murmur, usually of a temporary character. Irregular or intermittent cardiac action is frequently present. The pulse as a rule is soft and small, and may follow the disturbed cardiac action in being irregular and intermittent. Slight but definite swelling of the thyroid gland, with marked pulsation in the vessels of the neck, has

been noticed in some cases. I have also observed not infrequently a fine tremor in the limbs, resembling that present in Graves's disease, and subsiding on rest.

CONSTITUTIONAL AND INHERITED DISEASES.

Gout and rheumatism are common amongst the parents and other relatives of these patients, and although this fact is emphasised by some authors I have not been able to determine amongst my own cases that such a family history is so frequent as to prove a causal relationship. Acute rheumatism in its definite form, with endocarditis, occurred only once amongst my patients, although such conditions as tonsillitis, aching pains in the limbs, etc., which may be termed rheumatic, were of frequent occurrence. The similarity which exists between these patients and the subjects of the uric acid diathesis suggests hereditary gout rather than rheumatism. A neurotic parentage has been in my experience extremely common. Although the existence of any one constitutional disease has not been determined, an interesting fact is that several children in the same family may be affected. This points clearly to the existence of some special diathesis. Heubner has related the history of a family where three girls, aged respectively ten, twelve, and fourteen years, all suffered from cyclic albuminuria. In a family of five children Lacour found three members suffering from cyclic albuminuria; and Schön found two out of five in a family similarly affected. One of my patients had a brother who presented many of the general characteristics of the disease, and whose urine contained a slight amount of albumin, but I was not able to determine if it was typically cyclic. A sister of another patient was seen at the age of twelve years. She presented many of the general characteristics of the affection, and the urine occasionally showed a trace of albumin at midday, at other times it was apparently normal. In several other cases I have examined different members of the same family, with a negative result as regards the existence of albuminuria.

(To be continued.)

WITH MR. MACLEOD YEARSLEY'S OUT-PATIENTS AT THE ROYAL EAR HOSPITAL.

ON September 5th, 1900, the following cases were selected as being of interest.

Large Polypus.

Here is a man æt. 28, who had suffered with a discharge from the left ear for about a year, but had hitherto sought no treatment. When he first came to the hospital he carried a distinct smell of foetid pus about with him. On examination, a large polypus was seen protruding from the affected ear. This was treated in the following manner:—The ear was first carefully syringed with 1 in 20 carbolic lotion, and then an instillation of 10 per cent. cocaine in spirit and anilin oil introduced, the polypus being manipulated with a probe to ensure the solution reaching to the root of the growth. A quarter of an hour later the polypus was removed with a Baber's wire snare. The growth was an inch and a quarter long. Its removal was attended with very little pain. The ear was then syringed with carbolic, by which means much inspissated pus and foul-smelling *débris* was got rid of. A dressing of double cyanide gauze was introduced, and the patient given instructions to remove the gauze the following morning and have the ear syringed twice daily with boric acid lotion.

This interesting case shows well how long a discharge can be borne by a patient in this class of life without causing much inconvenience. This man has had his malady for over a year, and cannot even remember the circumstances which attended its onset. Next week we shall be able to see the extent of the mischief that has occurred; probably we shall find that there are several smaller granulations which have been kept from growing by the pressure of the large polypus. The future treatment will be to thoroughly destroy all tendency to the growth of granulation tissue, to promote good drainage, and to ensure perfect surgical cleanliness. If any caries of the ossicles or bony walls is found it will have to be removed by the curette, etc. Next week, too, I shall examine the nose and naso-pharynx for a cause for the middle-ear condition. I was parti-

cularly careful to give him explicit instructions as to the proper syringing of his ear, and to impress upon him that he cannot do it himself; one of the most common causes of failure in treating these cases is that the ear, which is only seen by the surgeon once a week, is neglected by improper syringing at home. The patient may be perfectly willing and anxious to do all that is necessary, but it is quite impossible for him to use the syringe himself, partly because he cannot properly reach the ear himself, and partly because he is afraid of causing himself pain. Attention to this apparently small detail will save much useless annoyance.

It is astonishing to what size patients will allow a polypus to grow without seeking advice; I have, on several occasions, removed such large growths. Once I met with two, in the right and left ears of the same patient, both measuring two inches in length.

The method of local anæsthesia employed is that recommended by Dr. Gray, of Glasgow, and it certainly worked very well in this case. One must, however, be cautious in one's deductions as to the action of local anæsthetics, for they will, frequently, under what are, apparently, precisely the same conditions, abolish pain in one patient and not in another. The personal factor in local anæsthesia always requires consideration, and, no doubt, it is everybody's experience that patients' ideas of pain and their capacity for bearing it vary very considerably.

Chronic Deafness due to Nasal Trouble.

This case, a woman æt. 38, first came here for treatment last August. She complained of deafness in both ears of seven or eight years' duration, with tinnitus "like a railway," and occasional vertigo. The left ear is the worse. Her hearing for the fifty-inch watch was, R.—4½ inches, L.—almost contact. The tuning-fork gives the reaction for middle-ear deafness, that is to say, when placed upon the vertex it is heard best by the left—the worse—ear, and Rinné's test is negative for both ears. The bone conduction is almost perfect. On examining her ears the left membrane is seen to be pale, thickened, and indrawn; the movement to Siegle's pneumatic speculum is not quite as free as it should be. The right membrane is also indrawn; there is a patch of calcareous degeneration in the upper and

anterior quadrant, and the lower segment is somewhat atrophied. To Siegle's speculum the movement of the ossicles is good and that of the atrophied part of the membrane is too free.

To what is the condition here seen due? She states that she is subject to frequent colds in the head. On looking into her nose it is found that it is the seat of old chronic inflammation; and at one time her turbinates were much swollen, and she will tell you that her nose always used to be stuffy, but is less so of late. That is because the inflammatory tissue is now beginning to contract and the nose is advancing to a condition of what may be called "pseudo-atrophic rhinitis," in contradistinction to true atrophic rhinitis. If the pharynx be looked at, it will be found to have partaken in the process, and is also atrophic. Post-nasal rhinoscopy shows the naso-pharynx to be free but containing retained mucus, the decomposition of which is the cause of the foul breath so noticeable. These cases are very common, and if only people would take as much care of their ears as they do of their eyes, they would be less so.

How is this case to be treated? What are the indications? When she first sought treatment, it was found that the Eustachian catheter improved her. This helps us in dealing with the case.

The indications are:

- (1) To improve as far as possible the condition of the nasal and pharyngeal mucosa.
- (2) To improve the ventilation and mucous membrane of the tympanum by inflation and medication.
- (3) To improve the movement of the ossicular chain by massage.

This being so, the first is provided for by directing her to use daily a nasal alkaline wash. This was ordered when the patient was first seen. Last week the hearing power was, watch, R.—4½ inches, L.—½ inch; after the catheter, R.—5 inches, L.—1 inch. This week it is R.—5 inches, L.—almost contact. The catheter is used, and a small quantity of paroline, which is a pure petroleum, and acts mechanically, injected through it, the result is, R.—10½ inches, L.—1 inch. She will now get a Basdon's Chloride of Ammonium Inhaler and use it morning and evening for ten minutes at a time, inflating the ears by Valsalva's method twice at each sitting.

A few words regarding the passing of the Eustachian catheter. Always pass it on the easiest side first, ascertaining that side by nasal examination. This will gain the patient's confidence. Never attempt to pass it without using the diagnostic tube. Always use the utmost gentleness, never use force; to do so defeats your object and may do damage. If there is any difficulty, pass it under illumination, through a nasal speculum. There are several ways of passing the catheter, the three following being the most common:

(1) Holding the instrument like a pen, pass it gently along the floor of the nose until you feel it impinge on the back wall of the pharynx, then turn it a quarter turn away from the ear and withdraw it until you feel it hook against the nasal septum, then give it a three-quarter turn out; push it gently forward and it will engage in the Eustachian orifice.

(2) On reaching the naso-pharynx, draw it forwards over the Eustachian cushion, and it will slip into the orifice.

(3) On reaching the naso-pharynx, draw it forwards, beak downwards, until it hooks over the edge of the hard palate, and give it a half turn outwards.

When the catheter is in the Eustachian orifice, the ring on the outer end, which indicates the position of the beak, is in a line with the outer canthus of the eye.

The third indication for treatment given was the massage of the ossicular chain. This is not so much needed in this case as it is in others, where the movement of the ossicles is more impaired. It is done by the alternate condensation and rarefaction of the air in the meatus with a Siegle's speculum or the more powerful masseur of Delstanche.

External Otitis.

This man, æt 35, came here last week complaining of a discharge from the ears which had lasted on and off for eighteen months. He was here three years ago for the same trouble, and was relieved. He has no deafness to complain of, and the discharge is preceded by irritation, for which he scratches the ear with a pin. If neglected the discharge smells unpleasantly. On examining his ears, they were found to be the seat

of an eczematous inflammation of the meatus. When one finds this condition the first thing to look for is a cause which may be traced to the middle ear. Both this man's membranes, however, were intact, and his hearing power is not affected when the ears are cleared of discharge. He stated that he has suffered with eczema of his hands and in other parts of his body; he says that his "digestion is not all that can be desired" and his bowels are constipated. The case is a very straightforward one and treatment has already been attended by success. He was given directions as to his general condition, with Apenta water every morning, and an application of Unguent. Zinci Oleatis night and morning. It will be seen that the ears have already greatly improved after a week's treatment.

Chronic Deafness with Troublesome Tinnitus.

When this case came a week ago his chief complaint was that of noises in the head. These, he stated, were of a "buzzing" character, and were most troublesome. He also complained of deafness and a slight discharge from the left ear. The hearing was for the fifty-inch watch, R.—0, L.—3 inches. Examination showed that the right membrane was destroyed and the left was the seat of a small anterior perforation, the rest of the membrane being white and opaque. Inquiry elicited the fact that he had suffered from a discharge from the right ear "years ago." There was chronic rhinitis and pharyngitis of some standing. The Eustachian catheter improved the left ear to four inches, but made no difference to his tinnitus. His pulse was of high tension, and his bowels were confined. Besides treating the ears, he was ordered an aperient mixture and a five-grain calomel pill. This week he tells us that his tinnitus is much better.

In dealing with cases of tinnitus one must always bear in mind that there are other causes for it besides purely aural ones; indeed, no man can successfully deal with diseases of special organs if he neglects the rest of the body.

Tinnitus aurium may be classed in three groups, nervous, entotic, and objective.

(1) The *nervous* originate in the labyrinth or brain.

(2) *Entotic* originate in the ear itself or in its immediate neighbourhood:—(a) from blood-

vessels; (b) from muscular contractions; (c) from movements of the tympanic membrane, Eustachian tube, or collections of mucus in the ear.

(3) *Objective* sounds are loud enough to be heard by others.

A *high-pitched* tinnitus (singing, hissing, chirping) indicates increased tension in the middle ear, and is due to pressure on the stapes, active or passive hyperæmia, acute or chronic catarrh (especially when the labyrinth is implicated).

A *low-pitched* tinnitus (rushing, humming, shell-like) is usually vascular or muscular, and is especially perceptible when the resonance is increased by cerumen, collections of mucus or pus, etc. When *pulsating* it is due to arterial congestion in the middle ear or labyrinth. A *rushing* or *buzzing* tinnitus is generally due to venous congestion, is worse on lying down, and is relieved by purgation. A deep *humming*, which is better on lying down and taking food, is caused by anæmia. Such noises as *scraping*, *crackling*, *gurgling*, are usually the result of movements of exudates in the ear.

The noise in the case under consideration was evidently due to venous congestion, as evidenced by the state of the man's pulse and his general condition. The old inflammatory trouble in his ears, with its resulting thickening of the mucosa and the probable presence in the left ear of some pus and exudate, increased their resonance, and thus rendered him cognisant of the venous buzzing more than he would have normally been.

Before leaving this subject, I would point out how much more frequent *pulsating* tinnitus is than patients would lead us to suppose. One can often prove this by counting the patient's pulse aloud and making him compare it with the noise complained of. Pulsating tinnitus can be localised to the middle or internal ear by pressure on the carotid or vertebral arteries respectively.

Mastoid Operation for Cholesteatomata.

This patient is a man æt. 20, for whom I have recently performed the mastoid operation, for cholesteatomata. Except that he will again report himself in October, this is, I hope, his last appearance here.

When first seen, he stated that he had had a discharge from the left ear since birth. There was occasional vertigo, and the hearing for the watch was contact only. On examination, the whole

membrane was found to be destroyed, and cholesteatomatous masses were found in the attic region. By careful intra-tympanic syringing much of this was got away, with resulting improvement. This improvement was maintained for a time, but fresh cholesteatomata appeared and some granulations, and the ear was twice curetted and washed out under cocaine. As the hearing did not improve, granulations recurred, and vertigo increased, it was decided to curette the ear under chloroform. This was done, much *débris* was cleared away, but no ossicles were found. The operation improved him for a time, but symptoms recurring, he at length consented to the mastoid operation. This was performed in May last. The bone was very dense and the antrum deeply situated. The Schwartz-Stücke method was employed. The carious remains of the incus were found, much cholesteatomatous material removed, and the antrum and attic thoroughly laid open. The cavity was swabbed out with pure carbolic acid, the usual flaps made from the cartilaginous meatus, the meatus well packed with gauze, and the wound sutured. He left the hospital twelve days later, and the improvement was rapid and well-marked. As he is now, he has had no discharge for over a month, there has been no vertigo, or any other symptom. On examination, a large, smooth, epithelium-lined cavity is seen, which is quite dry. What hearing he possessed at the time of the operation he retains.

The formation of these masses of cholesteatoma is one of the most troublesome complications of chronic middle-ear suppuration. It is due to an ingrowth of epithelium into the tympanum through the perforated membrane from the skin lining the meatus. This ingrowth, with inspissated pus and cholesterine crystals, collects in concentric masses in the tympanic cavity and antrum. The masses often lead to much dilatation of those cavities, and may even cause a perforation of the bony wall. Since they teem with micro-organisms, they may, when stirred up to activity by inflammation, give rise to temperature, pain, vertigo, and other symptoms. Owing to their nature, they swell with moisture, and therefore syringing with aqueous fluids must be avoided in their treatment. When small they may sometimes be got rid of by means of the intra-tympanic syringe, previously dislodging them

with a curette. They have, however, a marked tendency to recur, and, as their presence is an element of danger, radical surgical treatment by an operation like that done in this man's case is usually ultimately necessary.

Adenoids with Deafness and Chronic Middle-ear Suppuration.

This girl is nineteen years of age, and complains of deafness and a discharge from the right ear. She is undecided as to how long her trouble has lasted, and presents the usual face of one who has been an habitual mouth-breather for a long time. Her right ear is the seat of an anterior perforation and the left membrane is indrawn. The fauces present nothing abnormal, but both palpation with the finger and the posterior rhinoscopic mirror show a mass of adenoid growths in the naso-pharynx. You will note that the hearing is improved by the use of Politzer's bag. She will come into the hospital, I shall remove her adenoids, and she has a very good chance of recovering fair hearing.

There is no need for me to point out to you the necessity for removing adenoids in children, but I should like to emphasise its importance in adults. Possibly most of you have been told that adenoids disappear, or, at any rate, cause less trouble after puberty. Has the reason, if that statement is true, ever occurred to you? It is not that the adenoids disappear so much, as that the naso-pharynx develops into a much more roomy cavity in adults. Consequently the post-nasal growths look to the mirror, and feel to the finger, much smaller than they really are. It is as important to remove adenoids in adults as it is in children, for they are a pathological entity, and, as such, are always ready to act detrimentally,—with every attack of cold they swell and add to the obstruction in the naso-pharynx.

So much has been written about adenoids that I do not think I need say more as to their action. I have selected this case because I wished to point out to you what I have just indicated. If anything be needed to emphasise my words, the great improvement which attends their removal in adults is sufficient.

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WITH DR. SAVAGE AT BETHLEM ROYAL HOSPITAL.

II.

GENTLEMEN,—To-day we are going to consider cases of melancholia. By melancholia one means mental pain; that which is the equivalent of pain or tenderness in the body is met with in the mind. You have in the body several varieties of tenderness; you may have simply exaggerated self-consciousness and tenderness; you may have the tenderness of the man who, having had an inflamed joint from rheumatism or gout, is extremely sensitive, but nothing more; you may have a pain that is more exaggerated, so that the person's attention is entirely occupied; or you may have such a racking miserable state of pain that the man is unable to do anything, he is rendered stupid by pain. Similarly, in what we call melancholia, we have to do with mental pain, and the cases of melancholia have to be studied from both their bodily side and their mental side. In referring to such cases the other day I said that a very large number of the maniacal cases, especially those suffering from simple mania, were in fair health, eating well, perhaps largely, sleeping fairly well, were fairly muscular, with pulse and circulation and all the other functions normal. But when you come to consider acute and active melancholia you have to do with something else, you have to deal with mental pain associated with bodily disorder. A typical person suffering from melancholia has a feeble circulation, a feeble digestion, a bad appetite, and almost always constipation of the bowels. There is an inability to attend to ordinary matters of business. At the same time the sleep is very bad, so that the brain is badly nourished; the patient is badly nourished as far as digestion is concerned, as far as circulation is concerned, and as far as sleep is concerned. Respiration is generally slowed also, thus the whole of the vital processes are lowered, and the mental expression of lowered vitality is depres-

sion and melancholia. I shall have to point out to you that there are connecting links between simple defect of power and simple painfulness associated with this defect of power, so that you get one person with a feeble circulation who is stupid and foolish, who is unable to make up his mind about anything, he is will-less, and he is unable to act or to remember. You have certain others who are both melancholic and stupid, and you will see in the wards certain cases described as suffering from melancholia with stupor, people in whom there is a feeble reaction to all outside stimuli, at the same time that there is a painful impression with it. There is a kind of mental stiffness as well as mental pain, and I shall have to point out to you the various types of mental disorder that occur in these conditions of physical weakness.

What are the causes that you would expect to produce melancholia? They may be said to be three. There is first of all simply the physical. There is the man who has had the physical illness which reduces him to a state of weakness and misery. A man with melancholia cannot sleep, has no appetite, and has pain everywhere. He has neuralgic troubles, is badly nourished, becomes depressed and thinks he will never get well, that he is going to die, or that he has got some malignant disease. There may be some physical cause starting the whole of the mental depression.

Secondly, there may be a moral side. I saw yesterday a very typical case of a young fellow of nineteen who belonged to a neurotic family, was fairly clever at books, but rather soft in relation to his schoolfellows; he was an individual who was called a "mug" or "smug" or both. At all events he was less distinguished in the football and cricket field than he was in class work. He passed an army examination to the satisfaction of his friends, without going to a crammer, and went straight into an infantry regiment, which, as it transpired, had not a good reputation. This individual was at once looked upon as rather soft, and his brother officers began to play the fool with him. They made "hay" in his quarters, borrowed his bicycle and returned it smashed, damaged his uniform, destroyed his hat, destroyed his visiting cards and his papers; they also made additions to his name

which were not complimentary. He was worried, bothered, bullied. He passed into a condition of profound depression. Now he is quite sure there must be something wrong with him; he is quite sure he must have sinned beyond all other people or else he would not have been treated in such a way; he slowly passed into a condition of profound melancholia. So here was a case in which shock and worry and anxiety produced melancholia with the interpretation that he was an unpardonable sinner and that there was no hope for him in the future.

Thus you may have a cause that is purely physical; you may have a cause that is almost purely moral; or you may have many causes which include both physical and moral. A man loses some of his money or he retires from business and says, "Well, when I was in business I had two thousand pounds a year; but now I have retired from business I have only a pension of a thousand a year; how shall I make both ends meet?" There is truly a reduction of income, and there is reason for him to be more careful than he was before, but that is not enough cause for him to say that he is utterly ruined. A man whom I trust will be in an asylum tomorrow, whom I saw recently, was one whose business was precarious. As a consequence of the war and of his having been very liberal for benevolent purposes, his income was reduced to about one third less than what it was this time last year. Of course that is a real cause for anxiety, but it is not sufficient to make him wring his hands with despair, and pull out his hair and bite his nails. He says it is no good, it is all a mistake, and if they do not take care of him he will be obliged to kill himself. He says he will soon be in the workhouse; his family has been respectable, but now all that has vanished. Thus you see the causes of melancholia may be material, or may be moral, or may be both.

Melancholia may be the direct result of physical changes in the brain; it may be a direct result of changes in the general nutrition. There is an old gentleman whom I saw this week, and about whom his doctor wrote to me thus:—"For goodness' sake do not let him see me any more, or else I shall be like unto him. He comes back daily and says, 'I did not tell you this yesterday,' or 'I did not tell you that yesterday,' or 'I forget

whether I did tell you what I did,' and so on and so on till he drives me wild." I examined that man's urine and found two and a half grains of sugar per ounce of urine. He is suffering from malnutrition of the brain, and one of the symptoms of his diabetes is melancholia. You have also to remember that melancholia may be the result of brain disease. You will see later on some elderly gentlemen who are weak-minded as the result of age, and who are melancholy as the result of age, the wear and tear of brain has left them miserable and depressed. Other individuals are melancholic because their brains are badly nourished, as in that case of diabetes; in similar cases sometimes the malnutrition is associated with heart disease. There are some individuals to whom I must refer in passing who belong to the class that Sidney Smith called "lemon-juice squeezers,"—people who go on squeezing the acid and sourness out of their lives, and as they get older there is an increasing amount of sourness in life for them, and finally with age they can see nothing but misery and they become chronically incapable of fulfilling their duties to society. One is obliged to class these persons as melancholics, although it is a mere exaggeration of their type of mind.

Melancholia may be divided according to the interpretation or expression. One set of people attribute their pain to some bodily organ. One man says his brain is wrong; another man says his stomach is wrong; another says that his reproductive organs are imperfect or wrong. And so you have a person suffering from melancholia whose whole thought is centred upon the change of feeling in his brain. One patient says that the whole of his brain has run away; another that his brain is converted into some strange body, into some fluid that passed away with his water. Another individual said that God had struck his brain and annihilated it. But all of them give insane interpretations of uneasy feelings within their skulls. Others tell you they have some obstruction in their bowels, either at the pit of the stomach or at the rectum. These cases are often called hypochondriacal. Other melancholics have the one thought that there is some disease associated with their reproductive organs. We shall see presently one patient who will tell you that all his trouble depends upon his habit

of masturbation, in which he indulged as a younger man, and which he has never been able to recover from. The insanity rests in the bodily interpretation of all these disordered feelings. The patients feel this wretched depression, and while some say it is dependent upon some bodily disease, others will tell you that it depends upon some moral fault. I am in the habit of saying that the three groups of melancholics are those who suffer through mind, body, or estate. Some say they are going out of their mind, or that they have lost their souls; others say they have bodily obstruction or defect; others say they have lost all their money. There are others who have a general feeling of malaise. A Swiss governess who will be in this hospital in a day or two, whom I saw two days ago, is a very good example of how melancholia may grow, and how the symptoms may develop. A woman about thirty years of age was a governess in the house of a clergyman, a friend of mine, for some years, and was a thoroughly conscientious, hard-working, good woman, who was trusted with every detail, and she was working and saving in the most praiseworthy way. She was helping to keep an aged parent, while at the same time she was putting by money to enable her to marry. It was agreed that she should be married this spring, and she went to Switzerland to arrange about the wedding. Her lover said, "Are you prepared to find a couple of hundred pounds?" "No," was the answer. And then he said, "Well, I have taken a bigger business and I want more capital and we cannot be married unless you can provide the £200." She said, "I cannot, I am providing for my own father, who is living in France, and I must think of him." "Very well," said her lover, "We must wait two years longer, that is to say until you can provide the £200." That was a great disappointment to a woman of thirty, with the years passing over her, and the feeling that her chances of marriage were not increased with advancing years, and she began to wonder whether her lover was really attached to her. Here was a test whether it was her duty to sacrifice her lover or her father. Sleeplessness followed; bad digestion followed; loss of appetite and loss of flesh followed, and then came the idea "supposing anything happened and I died now." Then arose the fear of death. She was brought

to me in a very excited condition, believing that she was going to die and that she might die at any moment; or that her lover might die; or if she or he did not die her father might. So here we have physical disability with a mental interpretation.

You may have ample depression without any delusion, or you may have delusions as well as depression. Most of us recognise what simple depression is, and it varies in degree with the individual. Certain individuals are liable all their lives to be in the depths or in the clouds. Some of the most useful people I know are either doing twice as much work as they ought to at one time, or else they are not doing half what they should, bringing the average up to about that of an ordinary individual. There are certain persons of that type who are subject to periods of depression, and such periods of depression are periods of great danger; such people often as a result of the depression believe their souls are lost, their bowels are obstructed, or that they are going to the workhouse, and they may be intensely suicidal and may feel that life is not worth living owing to this mental pain which they suffer. The nature of the symptoms depends to a great extent upon the age, the sex, or the occupation of the patient. The young man becomes depressed and thinks that he has made a fool of himself; he is extremely self-conscious. An old man when he becomes depressed thinks he has wasted his life, that all his means have gone, and that he is going to a dishonoured grave or to the workhouse. Sex, again, makes an alteration with regard to the symptoms. A man thinks selfishly of his property, of his digestion, or of his bodily functions. On the other hand, a woman thinks she has been injurious to other people; that she has neglected her duty to God or man, to her husband or to her children; and she is more likely to be dangerous because she thinks she is in the way, rather than from dread of personal sufferings. Then, again, as to occupation. I am in the habit of saying that a parson naturally thinks his soul is lost; the doctor thinks he has got aneurysm or general paralysis, or cancer. I see such doctors constantly. Many come to me saying they are sure they have general paralysis. Those who become officers of asylums go through a stage of general paralysis in their own minds, just as, earlier, the

medical student goes through the phases of heart disease and aneurysm and so on. The more educated the man is, the more refined is the type of misery which he suffers. There are three or four points which you will have noticed in all cases of melancholia and which you must observe and never forget in examining people suffering from mental depression. First and foremost among these is their tendency to self-destruction. Remember, every person suffering from melancholia is a potential suicide. I shall point out to you that there are some people who are much more likely to commit suicide than others, but you must never forget that every patient who is brought to you despondent is a possible suicide, and it is therefore of the utmost importance that you should so regard every such patient. Again, there is the matter of taking food. The melancholic person is badly nourished, has feeble digestion, and as a rule has little or no appetite. If he also has ideas that his bowels are obstructed, that there is no possibility of his passing the food he swallows, he will use all sorts of means to avoid taking food, and he will starve himself if he can. Therefore one of the practical points is to see that all patients suffering from melancholia are carefully fed. Then probably the symptom which is treated most, and for which the majority of patients suffering from melancholia will come to you themselves, is absolute sleeplessness. You will have to try all sorts of measures to induce sleep. Sometimes you will have to give drugs, for you should remember that in many cases sleep is of the utmost importance. It may be well to read to you a letter which was handed to me as I left my house. A lady suffering from melancholia describes very graphically what her anxieties are:—"I am sorry to tell you that the terrible idea that I may have done some harm to a woman whom I passed on the cliff never leaves me. Only when I am asleep is it peace. It never leaves me all day long, and my brain feels wearing out with always thinking over the walk and trying to put myself right about it. I have always a dim remembrance of passing this woman along the cliff, and this torments me so. I cannot think why all these frights have come over me. If I could only be certain this is due to nerves which catch and torment me out of doors I should satisfy myself afterwards. But

it never goes out of my head that it may be true that something has happened to this woman, and that no one can help me by proving that it has not. I feel wearing myself out, perhaps for simply nothing, as my mother assures me it is all delusions. But though I tried hard, I cannot help myself, and feel that everything in life is over for me, and that I shall never be happy again, though I long to be. It seems so hard on me saying all these horrid things about myself, and to know it is through that dull winter in that quiet place where I was living. I do not care now for talking to people, as this does not seem to help me or take me out of myself. I do wish I had come up to see you some months ago, as I do feel so certain this is never going to leave me now. There is no chance of my ever being of value again. I hope you do not mind my writing so much as this to you, but I do feel so wretched. Some friends have asked me to go away, but I do not feel that I can. I do not want to keep talking to them. I have refused to go. It seems really as though something within me was killed, and that I am different from every one else." This is a very typical letter of a young lady suffering from a form of melancholia. Her idea was that everything that happened might have had something to do with her. She walks along a cliff at Dover, and a week afterwards she hears that somebody threw herself from the cliff at Dover. There are the two things put together: "I walked along the cliff; a woman threw herself over the cliff; was it anything I did or said, or any suggestion I made which offended that woman so that for that reason she threw herself over the cliff." Then she begins to hunt up and look through diaries that are carefully kept as to the date and hour she walked along the cliff, and then she finds on what day this woman walked along the cliff. If they do not agree she is not satisfied. She worries her mother to know whether it is possible. She takes up a newspaper and finds that some one is burnt to death, and she remembers that she carries a box of matches with her—everything unhappy that happens she seems to think refers to her. This exaggeration of self-feeling is a great characteristic of all melancholic states.

This man feels very unhappy, he says that his general health is ruined, and that he is unlike

other people; he thinks his intellectual feelings are dead. He sleeps fairly well; he dreams nearly every night, but the dreams are not unhappy ones. If a man is miserable by day and happy by night he will certainly get well. I see such people every day. I say, "How are you?" "Oh, I'm wretched." "Do you sleep badly?" "Yes." "Do you dream?" "Yes, I am just as bad asleep as when I am awake." That means that the patient is not improving up to that time. You may see the same patient a month afterwards and you ask, "Do you dream?" And the answer is, "Yes, and the dreams are such that I feel I do not want to wake again." I say to such patients that they will soon wake up happy again. One patient feels as if there were some bodily ailment about him. He thinks that the faults of early youth produced this condition, though he is a young active man. I am in the habit of saying, "Let the dead past bury its dead." He says if he once got thoroughly happy he would not think of that again; but now he cannot think of anything else. I believe that this general sense of feeling that he has done wrong will in time pass off, and one can see that there is nothing here which is irretrievably damaged. He says he feels weak. The pupils are of fair size. In conditions of neurasthenia or hypochondriasis of this type one frequently gets widely dilated pupils associated with feeble circulation, and feeble nerve reaction. In this case, however, there is practically nothing of that kind. The circulation is good, the pupils are of fair size and react, and the tongue is fairly clean. There is a good deal slightly wrong, but nothing seriously wrong, and I therefore assure him that he will soon get well. He should have sunshine, exercise, occupation—I was going to say also shower baths; but I do not think they are used in asylums now-a-days.

One of the types of hypochondriasis that you will meet with frequently is the hypochondriasis occurring in young men and young women who attribute their sleeplessness and their misery to masturbation. But I say in regard to these cases that masturbation has nothing to do with it, except from the moral point of view. They worry and worry, and of course enormous harm is done by the quack books which are preaching forever that because a boy at school learnt such a habit

that he is irretrievably lost, and that he will never be well unless he takes the quack medicine. I think far more harm is done by such books preaching admonitions than even by the most advanced teachers who are for ever preaching soul damnation. But now-a-days people do not accept that idea of soul damnation so readily as they do that of physical damnation as a result of masturbation. Individuals who are brought to me suffering from the idea say, "Have not I ruined myself by masturbation?" I say, "No, you are ruining yourself by the idea of masturbation." There is a young student at present under my supervision, and I see him now and then. He had the idea that he was irretrievably lost; that his mind would never come back, because of this habit. He took every precaution and mechanical protection against masturbation. He got an aluminium protector, which was strapped on to his pubis, and when he went to bed he had reels of cotton tied on his back so that he could not lie on it. He employed a man to watch him, and if the man took his eyes off him he discharged him and got another. I said to him if they were trustworthy men they were worthless, and if they were not watchful they were useless. Such watchers were mechanical beings who could be of no good to him. I went to see him, and he was in an awful state because one of the men took him out and he felt sure the temptation to the vice was so great that he wrote out a formal notice that he must not be allowed to put his hand into his trousers' pocket or lap. I said to him, "Till you can walk alone, and until you will exercise your will power you will do no good. As long as you are for ever thinking upon this one fault you are ingraining the very vice which you are dreading. Common sense tells you that if you go to the theatre you cannot be guilty of the fault; why do you insist upon having a man to walk always with you, even in the streets? Why should you insist upon having a man to accompany you here and there and everywhere. If you do not like walking alone, let the man leave you sometimes and then meet you again, thus compelling you to some independence of action. If you dare not act independently you will become a chronic hypochondriac without hope." You have to remember that such symptoms, if they last long enough, whether the ideas be those of

a lost soul or lost money, will become permanently fixed and will never be removed,—the patient becoming a chronic melancholic. I think this man we have just seen will get all right. One practical point which some of you will be asked is, should such a man be encouraged to go in for promiscuous sexual intercourse? Should they be married or not? You have no right to recommend promiscuous intercourse; you have no right to recommend such men to marry as a means of cure. If a man in this condition marries he will be too self-conscious to consummate marriage, and then he will be convinced that he is impotent, and he will probably commit suicide.

The next patient has been here two years, and has been all that time unhappy. The cause of his unhappiness is mental depression. He thinks his body is going to be injured; he does not know that anybody wants to kill him, though he has had the idea that somebody wanted to poison him. He says the food in the hospital tastes badly. He cannot say how a definite injury has been done to him. He does not sleep at all. There is such a thing as being conscious in sleep. There are a great many people who have melancholia who tell you that they never sleep, and yet the nurses and attendants at night will tell you that they sleep pretty fairly. But there is a difference between good sleep and bad sleep, just as there is a difference between good digestion and bad. I trust you are unconscious in sleep, and unconscious of having eaten and drunk, because in health you are without consciousness of function. Some people are painfully conscious for hours after they have taken food. They are nourished, but they are painfully nourished. So also there are people suffering from melancholia who sleep, but at the same time they do not rest properly and there is a kind of consciousness during sleep. So when a person tells you that he never sleeps, you may take it for granted that either his sleep is broken by dreams, or that it is the kind of conscious sleep which is not profound. Again, to attempt to compare the stomach and the brain, just as some people tell you that no sooner have they eaten their meals than they feel hungry again; so these people say, "I put my head upon the pillow and eight hours afterwards I am roused, but I have not the

slightest feeling of refreshment from my sleep." This patient thinks his food does not digest or his bowels act. He never writes home, and he does not manifest any interest in others; he merely thinks of himself.

Here is a single man æt. 53, with no fixed delusion, but he states he has been raised up to show forth the wrath of God; he believes he is anti-Christ, that he is damned, and that it is too late for him to be saved; he also thinks that his brain is all eaten up by worms. He does not sleep at night, and on several occasions he has attacked other people without provocation. He complains that snakes crawl over him and in his head, that all will power has gone, and he is very dejected and depressed. He does not seem to have had hallucinations of any senses except those of taste and smell. He thought that his food smelt and tasted badly, and also that his breath and skin smelt badly. He believed that his food had been poisoned. In this case the symptoms have lasted so long and become so fixed that the prognosis is distinctly unfavourable.

In the next patient there is some defective memory. There is a slow reaction to questions, so that it takes some time to elicit a response to the questions that are proposed. There is also an air of preoccupation. When you are suffering from pain of mind you do not give your mind to anything else. When you have toothache and you are asked whether you think of going to the theatre you say, "No;" you are sufficiently occupied by the one sensation. When a person is suffering from melancholia it is not one sense but it is all the senses which are invaded by this depression, so that a person suffering from depression is miserable and unhappy, but there is never *ennui*. When a patient is sent to Bethlem he may remain there a few months and do nothing except prowl about the area and grounds, and then have a meal, and then prowl about for an hour or two more, and then go in and look at the paper—not very much to do—there are concerts sometimes and bands sometimes, but not enough to occupy him. But if we were melancholic we should settle down to that routine and find it sufficient for us. That is an important thing to be remembered in the treatment of patients of this kind. In the early stages of melancholia it is not of the slightest use trying to stir them up.

I am constantly told by a patient's relative, "I have been doing all I can for father. I have taken him to the flower show, I have taken him to see the boat race, but I find him worse afterwards." I say, "Of course you do." In an active mental disorder you have no more right to stir a patient up than you have the right to stir up an inflamed joint or an inflamed eye. There is a stage in mental disorder when bed is infinitely better than anything active. I saw a patient yesterday who was desperately melancholic; his pulse was small and rather rapid; he had all the aspect of worry—dry lips, tremulous tongue—he could not sleep; his thoughts were for ever going round in one morbid circle. He was losing flesh rapidly, and he would not take sufficient food. The best thing to be done with that man was to put him to bed, have him carefully nursed, carefully fed, and employ a certain amount of massage; so that you are occupying, interesting, and feeding him, but not stirring him up: You will find far more people get well who are nursed than are stirred up. Nearly all cases of depression, if they sleep at all, do so in the early part of the night, and wake up in misery in the early morning. The period when mental and physical depression is deepest is probably between three and six o'clock in the morning, and it is at that time that the greatest amount of supervision needs to be exercised. One patient has a feeling of general melancholia, with vague delusions of all kinds, moral, material, spiritual, and bodily, with inability to occupy himself. He has to be looked after as far as feeding is concerned; but he is not restive. Do not take it for granted because a man is over sixty that therefore he is not likely to recover. When I was a younger man I was inclined to think that everything over fifty-five or sixty was senile, and therefore not curable. But the longer one lives, the less inclined one is to call one's self senile, and one looks forward hopefully to the older cases which recover. I am sure of this, that men and women who have power to live to sixty* or seventy or eighty have a reserve of energy that we do not quite reckon with; and I should say it is a very common thing to meet with old men and old women who have been condemned as suffering from senile mental changes, and who, after all, get well enough to live with their families. Of

course they are not quite as useful as they were. One finds as one gets on in years that one cannot run as one could at an earlier date, and climbing has to be done with caution. Nevertheless, because one does not run and jump and climb as one did thirty years ago one declines to be looked upon as a cripple. And so there are many people who have attacks of mental trouble and melancholia associated with age who nevertheless get perfectly well.

Our patient has led a thoughtful, earnest life; he has been a city missionary. There is some insanity in his family, but he has never had any attack before. There is no very special cause beyond age, worry, and influenza. Probably the life he was leading was rather a narrow and restricted one. With the influenza he became sleepless, and with the sleeplessness the brain became badly nourished and he began to fear some terrible catastrophe was going to happen to or through him. Therefore the patient got changed in appearance. He then said that he could not control himself, that he much feared he might do violence to himself or to others. He evidently had the delusion that the medicine he took was drugged and would be injurious. He fancied the food and the medicine were tampered with. At times he had visions of an alarming type. He felt that life was not worth living and that he had outlived his utility, so that the best thing for him to do was to end it. I have told you that suicidal symptoms are most important in relationship to some forms of mental disorder. Old people are distinctly suicidal. I shall refer to the relative amount of danger in different classes of patients. In the ordinary hypochondriac, as a rule, there is comparatively little danger. The hypochondriac who believes his bowels are permanently obstructed may be injurious to himself, directly or indirectly. I was called one night into consultation with a surgeon to see a governess who had been melancholic and whom I had seen once before. She had an idea that there was obstruction of her bowels and that the whole of her food had collected in her abdomen, so she was perfectly certain that some frightful catastrophe would happen if this was not allowed to escape; therefore she got a knife and simply ripped up the abdomen in six or eight directions. It was a most awful

wound, and I would have said it was impossible to do it one's self. There were three cuts from the pubes to the umbilicus, and there were six right across the abdomen, some of them going through the peritoneum as well. The night was spent in attempting to repair the damage, but it was not satisfactory and the patient died. A hypochondriacal person, as a rule, is too much occupied with his feelings to be suicidal.

To the next patient everything seems irksome, and he cannot take an interest in things outside himself. There is again in this case a slow reaction. In one hour and a half one hardly has sufficient opportunity of pointing out to you the difficulty of getting facts enough to put in a certificate regarding people who are suffering from melancholia. Their perception is dull, their reaction to questions is slow. And then in many cases, besides that, there is doubt as to whether they ought to answer, whether they may not be injuring themselves or others by answering. Therefore you have a physical retardation of reception of the impression and very much retarded reaction. And then you have the intellectual confusion due to uncertainty. So that when a person is suffering from melancholia, especially if it is associated with senile changes, you have great difficulty in getting sufficient facts quickly from the patient himself. One naturally asks how sleep is, and this patient says it is only moderate. He is seventy-two years of age, and was a member of the Metropolitan police. His profession may have something to do with the caution with which he replies to questions. A man's mental disorder is greatly affected by his profession. I have told you that the doctor is in the habit of thinking his body is wrong, and the lawyer thinks he has made some error in legal matters. A lawyer I saw some time ago was sure his professional reputation was ruined because he made use of one word instead of another, that is to say, "cause" instead of "action." He was seeking for a cause for his miserable feelings, and his professional training and judgment made him think it was something to do with this, and that it was this which was causing his trouble. Our patient's father committed suicide years ago while insane, and that of course has a very important bearing on this case. There is no doubt that melancholic types

or melancholic temperaments are transmitted, that in certain families you get a tendency to look at the black side of things transmitted from father to son. If a father has committed suicide, his son, when he gets depressed thinks, "I wonder if I shall ever do what my father did." So it is not only the transmission of the suicidal tendency, but the history of the father's action which plays an important part. There are certain individuals who are intolerant of suffering, who cannot bear suffering of any kind, and who seek the quickest way out of it by suicide. Such constitutions undoubtedly occur in the same family. I have seen families the members of which always become melancholic, and generally suicidal, when they attain to seventy or eighty years of age. Many of the longest lived families I know have a tendency to become weak-minded or melancholic at the end. This old patient tells us that he has had bad feelings come over him, as if he must do something. There was a scar in his throat caused by his having attempted to cut it with a pen-knife a short time before admission. His actions were strange and threatening before admission. Six weeks before admission he became worried and depressed. He had a good deal of trouble about engaging a servant, and seemed unable to decide what he ought to do; and then he became possessed by the idea that his food was being tampered with, that some one wished to injure him, or that some dreadful calamity would happen. With this depression he became suicidal, was sleepless, and did not take his food properly; he was not digesting it properly, and consequently lost flesh. Of course those symptoms in a man of seventy-two whose father committed suicide are not favourable.

What are you to do, on general principles, for such patients as we have seen. First of all you have to decide whether they are really likely to commit suicide or to attempt it. If you are sure that a patient is likely to commit suicide you have only one real duty, and that is to see that he does not. In nine cases out of ten that means that he has to be placed under restraint by certificate. Patients have to be placed under a certificate more frequently because they are suicidal than from any other cause. The friends are all very willing to accept the responsibility and to advise you what you ought to advise them; but

if any accident happens they take care to let you have the blame, which they will not share. They say, "If the doctor had said he was suicidal and therefore dangerous, and had advised us what to do, we should have followed his advice."

In the next place, if the patient is simply refusing food, you have to take care that the patient is kept in bed and thoroughly well fed. Patients are more easily watched if they are kept in bed than otherwise. Remember that the patient who is suicidal, as a rule, chooses one particular way to do it. A soldier blows his brains out; a doctor poisons himself; other men throw themselves in front of trains, as do also women, who often precipitate themselves out of a window or off a cliff. Melancholic patients who are suicidal will therefore be treated in bed better than anywhere else. And remember that when being treated in bed they should not be placed at the top of the house. Whether a patient is put on the top or on the ground floor may decide whether a life is saved or lost.

For the sleeplessness I am afraid drugs will have to be used, and one will have to point out to you what are the most valuable and useful. I am inclined to think that food and alcoholic stimulants given at night very often act as well as or better than bromides. Bromides in any case of restlessness and over-excitability are very useful. If there has been extreme sleeplessness, morphia is perhaps to be preferred to anything else. If you have a patient who is really suffering from several nights' sleeplessness, then probably a quarter of a grain of morphia, repeated in a few hours if sleep is not obtained, will be the best thing. The great point is to vary your hypnotics. The fashionable one now is trional, in fifteen- to twenty-grain doses. In this asylum sulphonal is used; in other asylums paraldehyde in drachm doses is preferred. It is rather a cardiac stimulant and therefore has certain advantages, the great disadvantage being that it is utterly distasteful, so that it is not surprising that people who have had two or three doses of paraldehyde say that they have been poisoned. In some cases of prolonged sleeplessness where the physical health has improved, general measures may be of service. For instance a very hot bath the last thing at night, or a hot bath with a certain amount of mustard in it, will give rest very much more

readily than almost anything else, and will break the habit of sleeplessness. In many cases you have to remember in feeding people and treating sleeplessness that you have to treat a habit. There are certain individuals who get a habit of refusing their food. One thinks of patients who would quietly come in with their heads down and sit down and open their mouths and allow the stomach pump to be passed. I remember going to a private asylum and the doctor said, "Excuse me for a short time; I am going to load the cannon." I found that the cannon was a canon of the Church of England who was fed regularly three times a day, and that was about the three thousandth time he had been fed by the pump. I think you must break a habit of that kind in the same way that you must break the sleepless habit. A good plan is to put salt in the food and drink, and stand a glass of milk at the side of the patient; the salt makes him thirsty and he drinks the milk. I was in the country some time ago when one of my old Guy's men said, "We have tried your dodge about the salt, and by that means we broke the habit of a patient of going without food, and we have had no trouble since."

Local Paralysis due to an Overdose of Camphor.—Dr. T. B. Greenley ('American Practitioner and News,' July 15th) records the case of a lady seventy-eight years of age, who took an unknown quantity of spirits of camphor. About an hour after taking it she became comatose, and finally appeared to be dead. Consciousness returned, however, after a considerable interval, and it was found on examination that her right hand and the right side of her face were paralysed. The patient retained the use of all the other muscles. She remained in bed for two weeks, sitting up subsequently for a few hours at a time. The appetite remained good. In four weeks she was able to walk about the room with assistance. At the time of reporting, some five months later, she could pick up a pin from the floor with the afflicted hand, and there was no perceptible trace of the facial paralysis. The treatment consisted of normal liquid of nux vomica and gentle massage to the affected parts.—*New York Medical Journal*, September 15th, 1900.

CLINICAL LECTURES

ON

THE VARIOUS FORMS OF INTRA-ABDOMINAL SUPPURATION.

Delivered at Westminster Hospital,

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LECTURE I.

GENTLEMEN.—It seems to me that with the opportunity which now presents itself of delivering several weekly clinical lectures, the best plan to adopt is to choose a subject of width and importance, to deal with it in its various phases, and to illustrate it by cases we have had in our wards during the past year. The subject of the various forms of intra-abdominal suppuration has been therefore chosen. With regard to clinical lectures, they should be of such a character as to be of permanent value for practice, and that this may be so a synopsis of this lecture has been prepared, and copies are now passed round. It is my intention to distribute a synopsis of each succeeding lecture, so that you may take away with you the gist of what has been said, and be enabled to retain a *résumé* of the subject.

Intra-abdominal suppuration is a wide subject, and you will see that it is divided into the *localised* and *general* forms. The most common form of localised suppuration is appendicitis, and we shall devote two or three lectures to that subject. Then we shall pass on to the consideration of subdiaphragmatic abscess, which is as rare as appendicitis is common, and then to perisplenic abscess, which occurs in connection with certain gastric ulcers. Suppuration in the lesser cavity of the peritoneum is a rare but exceedingly interesting form of localised intra-abdominal suppuration, and in the last ten years it has acquired a literature of its own. There are also included certain forms of tubercular peritonitis, because pus is often localised in sacs, and then there are the intra-pelvic forms of suppuration. Finally we shall deal with the general suppurative condition of the peritoneum known as acute general peritonitis. The connection which it is important to enforce between localised and general suppura-

tions is, that they are not necessarily distinct clinically, for most cases of general peritonitis commence in localised forms. Some years ago acute peritonitis was divided into idiopathic and secondary. The idiopathic form of peritonitis was said to supervene in children from cold. We have now come to recognise the fact that no form of acute general peritonitis occurs without there being, as a rule, some localised cause, or else some constitutional condition, such as Bright's disease.

LOCALISED INTRA-ABDOMINAL SUPPURATION.

I. *Appendicitis.*

Many of you have for some time been seeing numerous cases of this disease, and you will find that the synopsis will roughly focus the salient points. First, as to the nomenclature of appendicitis: there are several other terms which are being used as synonymous. Mr. Treves prefers the term perityphlitis; Küster calls it epityphlitis, while another surgeon styles it epiphyaditis. Appendicitis is not etymologically correct, but on the whole it is a term which is fully justified by use. Almost all inflammations in the right iliac fossa are due to morbid changes in the appendix.

In its clinical phases there is no disease so protean or so extraordinary in its nature. A case of appendicitis beginning very quietly may, within twenty-four hours, develop so alarmingly that the patient is soon seen to be suffering from acute general peritonitis, and perhaps in thirty-six hours is dead. On the other hand, a case beginning in an intense and acutely painful form may go on for two or three days in such a way as to suggest that it is one of acute general peritonitis almost from the first. Yet you may find that the temperature falls and the pain subsides suddenly, and the patient is well on the road to recovery in six days. Again, some cases begin in a mild way and then become suddenly acute, and the patient dies; others afterwards quieten down and end in recovery. It is therefore impossible to forecast at the beginning of an attack what the outcome will be, whether the patient will be alive at the end of a week, or the attack will be mild or severe, or the illness will prove to be of long or short duration. But for the purpose of clinical teaching one is obliged to

make a classification of appendicitis, although it is a disease in which classification may be regarded as harmful, because one may truly say that it is almost impossible to "pigeon-hole" the various cases met with. Moreover any particular attack may show, at various stages, the characters of several forms.

We shall commence with a description of simple catarrhal appendicitis. It is a catarrhal inflammation of the appendix, and is comparatively mild, but it certainly has very serious outcomes, because recurrent catarrhal appendicitis will bring about changes in the appendix which lead to destructive changes. Then there is the variety known as adhesive appendicitis. This is illustrated by the usual form of appendicitis which you see in the wards. A patient comes in with pain, vomiting, and constipation; you place your hand over the right iliac region and feel a hard lump, which is very painful. It is a solid exudation in the peritoneal cavity in the neighbourhood of the cæcum. The tongue is foul, the bowels constipated, the temperature is raised, and the abdomen is full. Resolution commences about the sixth to the tenth day, the temperature falls, the bowels act naturally, and the induration gradually disappears. Then you have appendicitis with abscess, which is a sequence of adhesive appendicitis, for a certain portion of the exudation breaks down. Then there is appendicitis with perforation or gangrene of the appendix—a very serious form. Finally, there are the relapsing and recurrent forms of appendicitis. A distinction should be made between these forms: Relapsing appendicitis means that the patient never is really well between the attacks; that he is always in trouble about his appendix and worrying about it, and his mind is always brought into relation with his appendix. In recurrent appendicitis the patient has an attack of appendicitis and then gets well for a time; but it may be that he is hurried, or takes something indigestible one night, and then he wakes up early in the morning in acute pain and he knows by the symptoms that he is in for another attack of appendicitis. So that one may say that in relapsing appendicitis the patient is never well between attacks, while in recurrent appendicitis he is.

Before I discuss the disease itself, it is well

that I should go over the anatomy of the various parts. The ilio-cæcal region is a region in which development has not taken place to its full extent, but has been partially suppressed. In herbivora, such as rabbits, the cæcum is a long one, but in man the proximal part of such a cæcum is represented by the cæcum itself, and the distal part is represented by the appendix. So that the appendix is really a structure in which development has remained stationary. You are aware from the history of certain forms of new growths, that if it happens that a tissue or structure fails to attain its full development, it is extremely liable to undergo inflammations and degenerations. This is precisely what occurs to the appendix. So that the appendix is not only subject to inflammations, but to malignant growths, although primary carcinoma is rare. As to the appearance presented by the appendix, you know it quite well, but there is one practical point with regard to operating. If you are searching for an appendix to remove it, it sometimes is by no means easy to find, as it is bound down by adhesions, and often tucked away. You should then define the muscular bands on the colon, for these are continued straight down on to the appendix. That is the case with one band in particular, the anterior muscular band. So you ought not to make any mistake between a dilated appendix and the ileum. You will be all the more careful if you think what are the consequences of making an error. The appendix is entirely covered by peritoneum, so that appendicitis is an intra-peritoneal affection. It is necessary to emphasise a fact which has important clinical bearings, namely, the appendix may take all manner of abnormal positions. The usual position is for it to hang down in the pelvis. In 51 per cent. of cases it is so placed; but it often happens that the appendix takes a different course. Often its course lies towards the kidney and its tip is just below the kidney. There is a patient in Queen Anne Ward who has a perinephritic abscess, and the pus has a fæcal odour, so that there is no doubt the abscess is due to a suppurating appendix which lay below the kidney and originated the suppuration in the neighbouring tissue. The rarer situations are towards the middle line. For instance, the appendix may turn towards the

middle line, and lie across the vena cava and the aorta. If the appendix is of undue length it may pass well towards the left side, and in such cases you have a left-sided appendicitis, although, as a rule, appendicitis is a right-sided affection. I dwell upon this point to show that in some cases you may have an appendicular abscess on the left side of the abdomen and not on the right. The appendix has been found below the liver, and even in the sac of a hernia, where it has become inflamed and suppurated. You have often heard of McBurney's point. It is a spot on the anterior abdominal wall which approximately corresponds with the insertion of the appendix into the cæcum. It is almost always situated at the mid point of a line drawn from the umbilicus to the anterior superior spine of the ilium. More accurately, it is two and a half inches from the anterior superior spine on that line. But the actual position of the appendix depends upon the position of the cæcum, and the position of the cæcum is not so constant as was at one time thought. In some cases the cæcum is high up, and in one instance it was found tucked well up under the liver. In that case the patient had an abscess, which was situated in the right hypochondriac region and well beneath the liver, so that the abscess was a subhepatic one. At the junction of the appendix and the cæcum is a valve, which anatomists call the valve of Gerlach. It is not really a valve, and is not of any value, being of anatomical rather than clinical interest. Messrs. Lockwood and Rolleston have described three fossæ of the peritoneum in that situation. You are asked to remember that there are the ileo-colic, the ileo-cæcal, and the sub-cæcal; but I do not think they are of great importance from a clinical point of view. The appendix has a small meso-appendix. This is of considerable pathological importance, because it varies much in length. If it be very short and the appendix become inflamed and thickened, it twists upon itself, so that a constriction is produced and cystic dilatation of the tip follows. I have already spoken of the muscular bands of the appendix. The normal length of the appendix is three and a half inches, but we recently operated upon a case in which it was seven inches long and passed

over the external iliac vessels into the pelvis. The healthy appendix should have no constant contents; it is only when it gets into the habit of retaining its contents that it is liable to inflammation. In passing I may allude to the various contents which are found there. The most common are fæces in various stages of hardness. Now one may imagine the following condition of things to occur:—If a patient has a considerable amount of fæcal matter constantly in the cæcum, a certain portion will lodge in the appendix. But if the appendix is quite healthy, when evacuation takes place and peristaltic movements of the large intestines are set going, the appendix expels its fæcal matter and remains empty for a considerable time. If the patient suffers from continued constipation, and these peristaltic movements are neither frequent nor vigorous enough, fæces which get into the appendix remain there, and water is absorbed from them, with the result that there forms what is known as a fæcal concretion or an enterolith. This condition continues until there is a large mass of hard fæcal concretion retained in the appendix. Sometimes in the appendix there have been found various foreign bodies, such as cherry-stones, strawberry seeds, oatmeal husks. Two cases are recorded in which pins were in the appendix. In several instances they have been found in appendicular abscesses; but what is remarkable is that an appendix was found in the sac of the hernia, which was strangulated, and in the end of the appendix was a pin.

Next with regard to the morbid anatomy and pathology of the parts. As to the cæcum, formerly the opinion was that all the inflammations in this region were due to the cæcum and not to the appendix. The modern idea is quite the opposite, namely, that all inflammations in that region, or practically all, are appendicitis, and that the cæcum has nothing whatever to do with it. As is always the case, the truth lies between the two. One may say that most of the inflammations in this region are due to the appendix, and that a few arise from ulcerative changes in the cæcum. Does typhlitis exist? By that I mean inflammation of the cæcum. I think it does, and I could quote you one case of mine in which I had strong reason to believe that typhlitis was present.

Then as to changes in the appendix. In what respects does it become altered? It is a useless pouch, but, as we know, fæcal matter can get into it, and it can not only get into it but stay there. In the appendix there is found a large amount of lymphatic tissue, because the Peyer's patches which you find in the large intestine are continued down into the appendix in the submucosa. Then there is a mucous membrane, with a great amount of absorbing power. Those who have studied the bacteriology of the appendix have found a very large number of the *Bacillus coli communis* and occasionally *Streptococcus pyogenes aureus*, and even *Pneumococcus*, so that you have in an appendix all the conditions for an inflammatory flare up; there are the foul irritating concretions, a mucous membrane which has an immense absorbing power, a virulent septic bacillus, and beneath the surface of the absorbing mucous membrane there is a submucosa with a large amount of lymphatic tissue, and in the neighbourhood there is a large lymphatic sac of peritoneum.

The appendix undergoes certain definite changes in disease. There is chronic catarrh of the mucous membrane, much the same as occurs in the mucous membrane of the larynx and nasal passages with an ordinary cold. The submucous tissue becomes thickened, and there are a number of inflammatory cells exuded in the submucous tissue and in the muscular tissue. These inflammatory cells organise and form new fibrous tissue, and that makes a gradually constricting ring round the appendix causing a cystic appendicitis. If this condition goes on throughout the appendix there is actual obliteration of the canal. If a man has a sufficient number of attacks the whole of his canal may become blocked up in this way. It is an extremely rare form, and its occurrence cannot be relied upon. Every now and then you do find, *post mortem*, cases in which the actual canal of the appendix has become obliterated, and when it is obliterated a man is not likely to have appendicitis. Lastly, there may be gangrene, either localised or total.

In the next lecture we will pass on to the more interesting ground of symptoms and diagnosis.

CYCLIC ALBUMINURIA.

BY

G. A. SUTHERLAND, M.D.,

Physician to Paddington Green Children's Hospital and
to the City Orthopaedic Hospital; Assistant Physician,
North-West London Hospital.

(Continued from p. 364.)

AGE OF INCIDENCE.

AMONGST my own cases the following table shows
the ages at which the albuminuria was discovered.

TABLE V.—Age of incidence in author's cases.

Aged 7 years	2 cases.
" 8 "	0 "
" 9 "	2 "
" 10 "	4 "
" 11 "	3 "
" 12 "	4 "
Total				15

Of sixty-one collected cases, including the
above, the ages at which the albuminuria was
discovered are as follows:

TABLE VI.—Age of incidence in 61 collected cases.

Aged 7 years	2 cases.
" 8 "	2 "
" 9 "	4 "
" 10 "	12 "
" 11 "	7 "
" 12 "	5 "
" 13 "	7 "
" 14 "	4 "
" 15 "	2 "
" 16 "	0 "
" 17 "	3 "
" 18 "	4 "
" 19 "	2 "
" 20 "	1 case.
Over 20 "	6 cases.
Total				61

Heubner's statistics of collected cases are shown
in the following table.

From birth up to 15 years	...	22 cases.
" 16 yrs. " 20 "	...	21 "
" 21 " " 30 "	...	10 "
" 30 " " 40 "	...	2 "
Over 40 "	...	1 case.
Total		56

The earliest age at which I have met with
cyclic albuminuria was seven years, and I am not
aware of any published case occurring at an
earlier period. The striking feature about these
tables is the preponderance of numbers during
adolescent life. The majority of cases would
appear to arise between the ages of ten and fifteen
years. Although cases occurring after the age of
twenty years are probably more frequent than the
above tables would suggest, they are not, as a rule,
met with amongst patients seeking medical advice,
but are accidentally discovered, as, for example,
at an examination for life assurance. So that it is
quite legitimate to conclude that amongst them
also the onset of the albuminuria had occurred at
a much earlier period.

SEX.

It is sometimes asserted that the "albuminuria
of adolescence" is essentially an affection of the
male sex. Marie's collected statistics of cyclic
albuminuria would support this view, as he found
forty-nine cases amongst males to six amongst
females. My own experience, and that obtained
from statistics I have collected, leads to quite
different conclusions. Of my own fifteen cases,
six were males, and nine were females; and of
sixty-two cases recorded by ten different authors,
I find that twenty-five were males, and thirty-seven
were females. This shows a distinct preponder-
ance of numbers in favour of the female sex.
These different results are probably due to the
fact that other forms of functional albuminuria
are mixed up with the cases of pure cyclic albu-
minuria.

RELATION TO OTHER DISEASES.

Cyclic albuminuria and previous nephritis.—Dr.
George Johnson was of opinion that all cases of
cyclic albuminuria might be traced to previous
attacks of nephritis. Senator has pointed out that
in the convalescent stage of scarlet fever with
albuminuria there is often a cyclic character about
the albuminuria. Reckman has described two
cases which bear on this point. In one, a boy of
ten years, who had suffered from diphtheria and
scarlet fever, each complicated with nephritis,
typical cyclic albuminuria was present without
any evidence of organic renal disease in the shape
of epithelium, blood-cells, or casts in the urine.

In another, a boy of fourteen years, who had suffered from scarlet fever six months previously, albuminuria had persisted, and was found to be cyclic in character, although the urine could never be described as absolutely free from albumin. He regarded this as a case of chronic nephritis, as white and red blood-corpuscles and casts were present in the urine. Dr. Pavy does not refer to the previous occurrence of scarlet fever in any of his cases. Dr. Heubner out of five cases of cyclic albuminuria noted a history of previous nephritis in two, but he found no evidence of organic renal disease when the patients were under his care. Of my own fifteen cases five had suffered from scarlet fever, and none from diphtheria. One of these, a girl of twelve years, had had scarlet fever at the age of nine, but her general health had not apparently been affected until after an attack of influenza in the following year. The second, a girl of seven years, had suffered from scarlet fever two years before coming under observation. The third, a boy of ten years, had scarlet fever seven years previously without any recognisable sequelæ. Two others gave a history of previous scarlet fever without any special complications. These five cases differed in no way from the other nine, in whom no history of scarlet fever was obtained.

It may be necessary to recognise that in certain cases during convalescence from acute nephritis, or in the course of chronic nephritis, there is a definite increase in the amount of albumin induced by the erect posture. Of such cases I have had no experience, but apparently they can be distinguished from those of pure cyclic albuminuria by two leading differences, namely, that the albumin is never entirely absent, even in the recumbent position, and that tube-casts and other evidences of organic renal disease are always present. They must be regarded as cases of renal disease, and do not call for further discussion here.

Pure cyclic albuminuria may occur after an attack of nephritis, but without other evidence of active renal disease, or it may occur without previous nephritis. It is well known that complete recovery frequently follows an attack of nephritis in childhood, and it cannot be admitted without further proof that a condition of cyclic albuminuria is evidence of previous nephritis or persistent nephritis. The occurrence of the two

conditions—first nephritis, and later cyclic albuminuria—would seem to be a purely accidental one, and the frequency of a history of scarlet fever, which disease is the precursor of the great majority of cases of nephritis in childhood, is probably not greater than one would find amongst other children. Of my own cases the five who had had scarlet fever did not apparently suffer specially from subsequent nephritis. Even if nephritis had existed in these cases it had certainly not persisted so far as clinical evidence could determine the point. Taking the whole fifteen cases, I was unable to determine in any one of them definite evidence of organic renal disease after careful examination, which was carried on for years in some cases. Such an experience is quite opposed to what one meets with in connection with any known variety of Bright's disease, and more especially the type which is characterised by little urine and much albumin. The view that cyclic albuminuria is necessarily a sequel or accompaniment of organic renal disease must be held as not proved, while a mass of clinical evidence is steadily being accumulated which tends to prove the contrary.

Cyclic Albuminuria and the Uric Acid Diathesis.

—The condition to which the latter term is applied is not uncommon in early life, and resembles cyclic albuminuria in having an indefinite pathology, but a well-marked symptomatology. Patients affected with this disturbance present many of the general characteristics and local disorders which have been referred to in connection with cyclic albuminuria. They are nervous, excitable, emotional children, of spare build, and of capricious appetite, subject to indefinite pains, to headaches, and to attacks of lassitude and depression. It was while studying this affection ten years ago that I met with the first case of cyclic albuminuria which had come under my notice. The description then given of the symptoms has been fully confirmed by Dr. Levison of Copenhagen, whose authority on the subject of uric acid troubles will be universally admitted. Further observation has supported the view that from the clinical standpoint there is a close connection between cyclic albuminuria and the uric acid diathesis. Cyclic albuminuria is a symptom which not infrequently accompanies the uric acid diathesis; while in any given case of cyclic albuminuria we

may confidently look for other symptoms of the uric acid diathesis. In other words, the uric acid diathesis may be present without cyclic albuminuria, but cyclic albuminuria is always accompanied by other signs of the uric acid diathesis.

Cyclic Albuminuria and Graves's Disease.—Here also we meet with an affection in which the pathology is still undetermined, while the symptomatology is very clearly defined. Certain similarities in the symptoms accompanying both Graves's disease and cyclic albuminuria led me to examine into the form of albuminuria, usually described as intermittent, occurring in the former affection. In both cases the patients are nervous, excitable individuals, with signs of vaso-motor disturbance. These are manifested in the sweatings and flushings, the erythematous eruptions, the pulsating large vessels, and the attacks of transitory œdema, more especially about the eyelids. Other symptoms of Graves's disease which had been noted in some of my cases of cyclic albuminuria were tachycardia, pigmentation of the skin, and fine tremors in the extremities. Two sisters, aged respectively seventeen and eighteen years, suffering from well-marked Graves's disease with albuminuria have come under my notice. In both of them cyclic albuminuria, as described above, was determined without any evidence of organic renal disease. One of them was kept at rest in bed for some days and albumin was entirely absent during that period, reappearing soon after she rose. Another case of Graves's disease, a lady aged fifty, who had albuminuria, was also examined. The result was that the urine passed on rising was found to contain the merest trace of albumin, while that passed three hours later contained a considerable amount. That the connection in these cases was not purely accidental will be shown by the perusal of a paper written by Dr. Warburton Begbie in 1874. In this paper he pointed out for the first time the occurrence of intermittent albuminuria in Graves's disease, and noted that the greatest amount of albumin was usually passed after breakfast. His description corresponds closely with what is now known as cyclic albuminuria. From the experience of these cases of Dr. Begbie's, and the ones referred to above, it seems probable that cyclic albuminuria is the form of intermittent albuminuria present in certain cases of Graves's disease, and this observation, if confirmed, may throw some light on the pathology of both affections.

(To be continued.)

THE DISCUSSION ON ANÆSTHETICS.

To the Editor of the 'Clinical Journal.'

SIR,

A young lady friend of mine took chloroform, and died under the influence of the anæsthetic. The administrator, a brilliant young physician, after futile efforts at restoration committed suicide. I have so often seen children moribund from chloroform that I have concluded that there is no sound in nature so sweet to the ear as the cry of a child on recovering from apparent death from chloroform. Why should we give chloroform? It is a lethal agent, and ether is so incomparably safer that it is a wonder that it has not displaced its rival. Ether is, contrary to the generally received opinion, a much more rapid anæsthetic than chloroform. I have operated on five children for squint under ether in a little over twenty minutes. I smother the patients with a felt cone containing wool saturated with the anæsthetic, and in thousands of cases I have never seen the alarming *deliquium* which is so common with chloroform. Combinations of chloroform and ether are mere mechanical mixtures (as my friend Dr. Beckett Truman, the analytical chemist, has demonstrated), and are, I think, to be deprecated, because we never know which of the two anæsthetics is responsible for the patient's condition. A mild, rapid, and innocuous anæsthetic is a great desideratum. I recently operated on a gentleman for cataract in his ninety-fifth year. Notwithstanding cocaine, he was not to be controlled, and, although the operation was happily completed without misadventure, there were great difficulties and drawbacks, which would have been obviated by a mild anæsthetic. Chloroform is too dangerous, ether too exciting and too apt to cause bronchitis, while the vomiting which so commonly follows either or both anæsthetics may spoil the operation. In such cases it is only a few moments' insensibility that is needed; the preliminary incision may be completed under cocaine; the cataract even may be extracted; but when it is necessary to complete the toilet of the eyeball, to remove cortex, replace the iris, and wash out the anterior chamber, it is necessary for the patient to be quiescent; indeed, if he is not, he may easily reflect the flap, rupture the hyaloid membrane, and let out the vitreous humour.

Dr. Flux's practice of giving nitrous oxide with air seems the most feasible method of administering an anæsthetic in such cases, and it would be interesting to learn how he is progressing with a system which to me seemed full of promise.

CHAS. BELL TAYLOR, M.D.

Nottingham.

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A CLINICAL LECTURE

ON THE

DIAGNOSIS AND TREATMENT OF INTRA-CRANIAL TUMOURS.

Delivered at the Medical Graduates' College and Poly-clinic, Chenies Street, W.C., September 12th, 1900,

By BYROM BRAMWELL, M.D.Ed.,
F.R.C.P.Ed., F.R.S.E.

GENTLEMEN,—Let me first express my sincere thanks to the authorities of this College for asking me to lecture before you to-day. I understand I am expected to deliver a clinical lecture. Of course different men have different ideas as to what a clinical lecture ought to be, but to me it seems that a clinical lecture ought, if possible, to be founded on some clinical case or cases, and illustrated by those cases or by specimens from them. I am unable to bring cases before you, living, as I do, at a considerable distance; but I have brought a preparation, in the shape of a skull-cap from a case of Jacksonian epilepsy, which illustrates very many interesting and important points in connection both with the symptomatology and the treatment of brain tumours.

In the first place, I will detail the more important features of the case, I will next consider the diagnosis, and finally, describe the appearances which were found at the operation—for the case was operated upon,—and also the post-mortem "findings," drawing, as I go on, certain general deductions with regard to the points which the case illustrates.

The patient from whom this skull-cap was removed was a man *æt.* 54, by occupation an engine-man, who was admitted into the Edinburgh Royal Infirmary under my care on October 4th, 1897, suffering from left-sided Jacksonian epilepsy of some five months' duration. The history that we obtained from him was that he had always enjoyed good health, with the excep-

tion of very occasional attacks of giddiness and slight headache, which he had experienced during the past three years. Headache was never, however, a feature of the case. Some twenty-three weeks before he came under my notice, while engaged in throwing some stuff out of a barrow over a wall, he was suddenly seized with spasm affecting the left arm and left leg. This convulsive seizure, which was not attended with loss of consciousness, lasted half an hour. He was carried home while in the fit. The same evening he had two more attacks, and from that date onwards until he came under my notice he continued to be affected with the same form of spasms, typical attacks of Jacksonian epilepsy affecting the left side of the body (arm and leg). There was no history of syphilis, nor of head injury, nor was there anything in the hereditary tendencies or family history to point to tubercle, or to throw any light on the nature of the case. When he came under my notice he was very considerably emaciated, and had lost two stones in weight. He was then having these attacks of Jacksonian epilepsy every six minutes, or at the rate of ten an hour on an average, and this sort of thing had been going on for several weeks as long as he was awake.

On examining him carefully we found that his left arm was paralysed, that he was weak in the left leg, that there was some exaggeration of the knee-jerk on the left side, but that there was no ankle-clonus. At that time I was not acquainted with Babinski's plantar reaction, and therefore I have no note of what the condition was in that respect. As far as we were able to observe, the attacks of spasm commenced either by contraction of the biceps, of the deltoid, or of the trapezius. The mode of onset did not appear to be invariably the same, but one of these three muscles, and in most instances the biceps, was always first affected. The march of the spasm from the point first affected was down the arm and then to the leg; after continuing for some three minutes, the attack passed off in the reverse direction to that in which it had commenced, the spasm ceasing in the leg before it ceased in the arm. During these attacks of spasm the patient complained of severe pain in the muscles affected by the spasm; and, as I have already said, after the attack was over the arm which was most affected was practically

completely paralysed. He could flex and extend the elbow-joint slightly, but he was unable to make any movement with the fingers or the wrist, and he could not raise or adduct the arm from the body. Careful examination failed to show anything indicative of a coarse cerebral lesion. There was no headache. I have mentioned that for three years he had occasionally experienced slight attacks of giddiness and headache, but when pointedly questioned he denied that he was suffering from headache at the time of his admission to hospital, or that he had suffered from headache during the twenty-three weeks that he had been ill. There was no tender spot to be detected on the head; there was no vomiting; and, a most important point, there was no double optic neuritis. On ophthalmoscopic examination it was thought that the left optic disc was slightly redder than the right; but that is an observation which of course it is very difficult to rely upon. Certainly there was no oedema and no optic neuritis. The temperature was subnormal (97°), the pulse 90.

Those were the facts of the case, and on those facts we had to make a diagnosis and determine the prognosis and treatment. The case was a typical one of Jacksonian epilepsy, *i. e.* of localised convulsions beginning in a very definite way and extending in a very definite order, always of the same character, unattended by any loss of consciousness, recurring with very great frequency, and associated with paralysis in the parts most affected by the spasm.

In a case like this, in which there are no positive symptoms and physical signs indicating what the exact nature of the lesion is, the diagnosis has often to be made by the method of exclusion.

The first step in the diagnosis was, of course, that the patient was suffering from an "irritative" lesion involving a very definite area of the motor cortex on the right side of the brain, a "discharging" lesion which corresponded to that part of the motor cortex in which the movements of the left biceps, deltoid, or trapezius are represented, in short, the upper part of the arm area. We know very definitely now what that area is, the motor cortex having been mapped out by Ferrier and others with the very greatest exactitude. There was no difficulty

in arriving at that point in the diagnosis, namely, that there was an "irritative" lesion in the immediate neighbourhood of the biceps centre. But when we came to try to make up our minds as to what was the nature of the lesion which was irritating the cortex and producing that discharge, the matter was a very much more difficult one. And here let me say that in dealing with a case of this sort, a wide pathological and clinical experience is of the very greatest importance. What one has to do, in order to arrive at a diagnosis, in a case like this, is to place before one's mind the different possible conditions which may produce the group of symptoms which are present, and by the method of exclusion to arrive at an opinion as to which of the possible lesions is the most likely or probable lesion, in the particular case under observation.

Now a great number of different lesions—organic lesions I mean, for in this "case" the question of a functional change did not come into account—may produce an attack of Jacksonian epilepsy. In the first place, the most common of all unquestionably is, I think, cerebral tumour; but in most cases of cerebral tumour there are very definite symptoms to guide one (headache, vomiting, double optic neuritis, etc.), and those symptoms were not present in this case. I shall refer to this point again presently.

The next, and it is a comparatively common cause of Jacksonian epilepsy, is a depressed fracture. Years ago I recorded an extremely interesting case of this sort which came under my notice in the Newcastle Infirmary; in it a speculum of bone, one third of an inch long, was pressing upon and irritating a very localised area of motor cortex and causing right-sided Jacksonian epilepsy, the spasms always commencing in the platysma on the opposite side. The case occurred before the operative days of cerebral surgery. The question of operation was, indeed, considered, but delayed at request of the surgeon who was consulted, and before it was carried out the patient died in a fit. After death we found that the area of irritation, which was very sharply defined, exactly corresponded with the area mapped out by Ferrier as the centre for the platysma in the monkey. Now in the vast majority of cases of Jacksonian epilepsy due to a depressed fracture, one has a history of an

injury to guide one, and perhaps some evidence of that injury, in the form of a scar or depression on the surface of the cranium. In the case I am describing, there was no history of injury and no indication of former injury on the exterior of the cranium. We were able, consequently, to exclude injury very definitely. In passing, let me emphasise at this point the importance of shaving the head, with the object of detecting the presence of swellings, scars, etc., in all cases of suspected intra-cranial tumour.

There are a number of other conditions which may also produce an attack of Jacksonian epilepsy; one of these is abscess of the brain. I have seen very definite Jacksonian epilepsy due to this cause. But here, again, in the great majority of cases you have a very obvious cause of cerebral abscess, such as ear disease, present, together with symptoms such as headache, very usually vomiting, and often some changes in the optic disc, with in some cases well-marked alterations of temperature. It is exceedingly hazardous to diagnose an abscess of the brain if there is no obvious cause for abscess present. Such cases do, however, occur. I have myself reported a very remarkable case of abscess of the brain for which there was no obvious cause. In that case we considered the advisability of operating, but because there was no apparent cause for abscess we decided not to operate. We should have operated; but, I repeat, it is exceedingly rare to have an abscess of the brain without an obvious cause, either in the ear or the skull bones, or the lung, or some other distant part; and, that to diagnose an abscess of the brain in a case in which there is no obvious cause for abscess, is not to make a diagnosis, but to make a guess.

Now in this particular case the fits had been going on for four or five months, and it is practically inconceivable that an abscess of the brain which was giving rise to so much irritation could have existed for that length of time without producing other symptoms. We were therefore able to exclude abscess.

Another possibility was meningitis, which may, of course, produce Jacksonian epilepsy, particularly syphilitic meningitis round a gumma. But that practically amounts to an intra-cranial tumour.

Tubercular meningitis sometimes begins with

local Jacksonian epilepsy. I have recorded a case of this sort which occurred when I was connected with the Newcastle Infirmary; in it, a man while working in a field was suddenly attacked with Jacksonian epilepsy, the spasms affecting one hand. He let the spade fall out of his hand. After that he continued for several days to suffer from Jacksonian epilepsy; his temperature then began to rise, and he developed the ordinary symptoms of a typical tubercular meningitis. I repeat, that in some cases in which there is a localised collection of tubercles, not a definite tubercular tumour, on the motor cortex, Jacksonian epilepsy is developed as the first symptom, before the ordinary symptoms of tubercular meningitis become apparent. That, however, was obviously not the cause here; the case had been going on too long, there was no evidence of tubercle in any part of the body, the family history did not point to tubercle, and the man's age, fifty-four, was rather against tubercular meningitis. Moreover he had not any temperature—practically speaking, chronic meningitis without any rise of temperature, unless it is syphilitic meningitis, does not occur,—and, as I have said, syphilitic meningitis with a gumma is, practically and clinically speaking, an intra-cranial tumour, *i. e.* is attended with the ordinary symptoms of an intra-cranial tumour. Further, in this case syphilis could be excluded.

In some cases of general paralysis of the insane one gets attacks of Jacksonian epilepsy. I have seen several cases of that sort. But here, again, there were no symptoms, either mental or physical, of general paralysis. We were definitely able to exclude that condition.

Pachymeningitis hæmorrhagica is another rare cause of Jacksonian epilepsy, but there did not seem anything in this case to point to that condition as a cause.

Then we also sometimes find that in some cases of ordinary idiopathic epilepsy the attack commences with a localised motor discharge, in short, with a motor aura. These cases are often very puzzling if the patient has not previously had any fully developed fits; but by observing the course of the case for a few months, and by taking into account the condition of the patient's health between the attacks, one is able to arrive at a correct diagnosis in that condition. In the

intervals between the attacks the general health is good, and if the localised motor spasms are merely the beginning of ordinary epilepsy, attacks of *grande mal*, fully developed fits, are certain sooner or later to occur. There was no suspicion that the case I am describing was one of ordinary epilepsy beginning with a motor aura. The patient had never had a fully developed epileptic fit with loss of consciousness; he simply had very frequently recurring attacks of localised tonic and clonic spasms affecting a definite part of the body, without loss of consciousness.

In uræmia we occasionally get localised spasms, more or less closely resembling attacks of Jacksonian epilepsy, but there the condition of the urine and the other evidences of kidney disease show at once what the true nature of the case—the cause of the spasms—is.

The last condition to which I shall refer in this connection is a cicatrix on the surface of the brain which has been latent for a long time and which has been roused into activity by some inflammatory or other process. That is an exceedingly difficult condition to diagnose as the cause of Jacksonian epilepsy; but in cases of this kind one has either a history of head injury or of syphilis, and a history of previous head symptoms, it may be long years before. I have described a case of that sort affecting the visual centre. The patient, who had syphilis and head symptoms many years before, came under notice on account of attacks of flashes of light, he said, in one, the right, eye, and these flashes of light were followed by temporary right-sided hemianopsia. In that case there was obviously a discharging lesion affecting the left half-vision centre. The man was suffering from malignant disease elsewhere, from which he died. On post-mortem examination, we found a scar, the result of an old syphilitic lesion, at the tip of the left occipital lobe. Now if such a scar, the result of syphilis or injury, or any other condition, were present on the motor cortex, and if, years afterwards, it should be excited into activity, or if any inflammatory changes should be developed round it, it would produce Jacksonian epilepsy, and, I repeat, it might be exceedingly difficult to diagnose the condition. But there was nothing in the previous history of the patient in the particular case which I am describing to cause such symptoms.

Now, considering all the facts of this particular case, we were able to exclude with tolerable certainty, I think, everything except tumour. The diagnosis arrived at was that the lesion which was causing the irritation was an intra-cranial tumour. Intra-cranial tumours, as every one is aware, may give rise to an immense variety of different symptoms. This is one of the reasons why the subject of intra-cranial tumours is of so much interest; in my opinion it is one of the most interesting subjects in the whole range of medicine. One may, I think, group cases of intra-cranial tumours into four great classes from the point of view of their symptomatology. In the first place there is a very small group in which a tumour has been present for many years without producing any symptoms at all. Such was the condition in the case of abscess of the brain to which I have already referred. The patient was a strong healthy man, who was suddenly seized with an epileptic fit, became paralysed and aphasic, and died in ten days from an evidently rapidly advancing cerebral lesion. Tumour was one of the things thought of as a possibility, abscess was another, cerebral thrombosis with softening was a third. I have already said that we rejected abscess because there was no apparent cause for abscess. We rejected tumour because the patient had been absolutely well until the commencement of the symptoms; he had never previously had any head symptoms whatever, and had not suffered from syphilis. We thought the case was one of cerebral thrombosis with softening, and for that reason decided not to operate. The patient died, and we made a post-mortem examination. As I have said, we found a very large abscess, full of stinking pus, in the centrum ovale on the left side, and in addition to this abscess there was a firm old tumour about the size of a walnut; it must have been present for many years without producing any symptoms. That is an extremely rare condition. Such a tumour is, of course, not diagnosable.

In the second group of cases of intra-cranial tumour there are very well-marked symptoms of a general kind, viz. headache, vomiting, giddiness, and double optic neuritis, sometimes generalised epileptic fits, but no localising symptoms. Every one is acquainted with these cases. In this group are included a very large number of cases of

intra-cranial tumour; the cases in which there are merely general symptoms without any localising symptoms form a very considerable proportion of the whole. Perhaps it would be more correct to say, that in many of these cases there are no localising symptoms sufficient to enable one to locate the tumour, rather than to say there are *no* localising symptoms. Of course one man may be able to locate a tumour from symptoms which might not strike another observer as of the least importance. As our knowledge of intra-cranial tumours increases, in consequence of the detailed and careful study of the significance of minute, and what may to some minds appear to be very insignificant, symptoms, there is no doubt that our power of localising intra-cranial tumours will become more and more perfect. But in the present state of our knowledge there is a large group of intra-cranial tumours in which there are general symptoms, but in which there are no symptoms which enable the most experienced physician to accurately locate the tumour.

Then there is another group of cases, which is also very large, in which in addition to well-marked general symptoms (headache, vomiting, double optic neuritis, etc.), there are well-marked localising symptoms, such as localised paralysis, or localised spasm, or hemianopsia, or other well-defined localising symptom, which enables one to say not only that there is a tumour, but to say with exactitude where that tumour is situated.

There is also a fourth group, which is larger than the first one, but much smaller than the other two, in which there are no general symptoms, but in which there are only localising symptoms. In these cases there is no headache, no vomiting, no giddiness, no optic neuritis, but there is, as in the particular case I have described, some well-marked localising symptom, such as Jacksonian epilepsy.

Now in the case which I have brought before your notice there was practically no headache—twice during the patient's stay in the hospital, extending over three months, he did complain of headache, but one of those occasions was after the operation—there was no vomiting and no optic neuritis. He had, however, well-marked localising symptoms, which enabled us to say where the tumour was. By the method of exclusion one arrived at the conclusion that the dis-

charging lesion which was situated in the upper part of the motor area for the left upper arm, was in all probability a tumour; the whole facts of the case seemed to show that it was much more likely to be a tumour than any of the other conditions which I have enumerated. In this case there was no question of hysteria.

The third step in the diagnosis concerns the pathological nature of the tumour. That is a very important point in certain cases for the purposes of prognosis, and particularly of treatment. Here, again, one's knowledge of pathological probabilities and possibilities is most important. We know, as a matter of practical experience, that certain tumours are common, particularly at certain periods of life and in certain situations, whereas other tumours are rare. The most common forms of intra-cranial tumour are gliomatous or glioma-sarcomatous tumours, syphilitic tumours, tubercular tumours, and cancerous tumours. There are other rarer forms, such as fibromata, but their rarity makes them of far less practical importance than those I have mentioned. In this particular case we could exclude syphilis, because the patient definitely stated that he had never had syphilis, there were no obvious indications of former syphilis on the surface of his body, and the style of the symptoms was not such as to suggest syphilis. But here let me say that Jacksonian epilepsy is perhaps more frequently due to a syphilitic than to any other form of tumour. When I say that the style of symptoms was not such as to suggest syphilis I am not referring to the Jacksonian epilepsy alone, but to the features of the case as a whole. I have never personally seen a case of syphilitic tumour with well-marked Jacksonian epilepsy existing for any length of time without headache as a prominent symptom. In syphilitic tumours headache is usually a very important and prominent symptom. In many cases, too, there is well-marked tenderness of the head as well; and very usually, also, double optic neuritis and vomiting. Further, in syphilitic cases the symptoms are usually relieved by anti-syphilitic treatment, at all events for a time. Now before this man came into hospital he had been ill five months, and he had been treated by iodide of potassium. For all of these reasons we were able to exclude syphilis.

There was no suspicion of tubercle; there was no history of tubercle in his family and there was no evidence of tubercle in himself, either at the time of his admission to hospital or at any previous period of his life. His age (fifty-four) was also against tubercle; and so was the long duration without other symptoms, and the whole course of the case.

There was no suspicion or evidence of primary cancer outside the cranium.

Here, again, in arriving at a pathological diagnosis, we had largely to rely upon the method of exclusion. We came to the conclusion that the tumour, in this case, was a glioma, and that it was probably of small size, this conclusion being based on the fact that the tumour did not appear to be causing any marked increase of intra-cranial pressure, as evidenced by the absence of headache, vomiting, and optic neuritis. I may here say that the more I see of intra-cranial tumours the more I associate optic neuritis with increased intra-cranial pressure as its chief cause, or, at all events, one of its chief causes. I have now seen a considerable number of cases of intra-cranial tumours operated upon, and in the cases in which the optic neuritis was very slight, or absent altogether, there has been no increase or very little increase in the intra-cranial pressure. I attach very great importance to increased intra-cranial tension as a cause of the general symptoms.

We came, then, to the conclusion that this man was suffering from a small glioma affecting the upper part of the motor area for the arm on the right side of the brain.

We had next to decide upon the prognosis and treatment. The prognosis in such a case, unless the tumour can be cut out, is, of course, extremely bad, as bad as it can well be. Viewing the lesion as a glioma, an operation seemed clearly indicated, but I always like to try the effects of the vigorous administration of iodide of potassium and mercury before an operation is performed, even in a case like this, taking care not to continue the administration too long. In this case I have told you that the patient had been treated with iodide of potassium before his admission to the hospital; he was again put on full doses of iodide of potassium, together with bromide of potassium and chloral hydrate, with the object of relieving the spasms. This treatment was con-

tinued for a week, but without any result whatever. The frequency of the fits was slightly diminished by the bromide and chloral, but the general condition of the man got worse instead of better; his leg became more paralysed, so it was obvious that the treatment was not doing him any good. He was, therefore, transferred to the care of my surgical colleague, Mr. Cotterill for the purpose of operation. He was operated upon by that gentleman a week after he left my care; he came under my care on October 4th, he was transferred to Mr. Cotterill's care a week afterwards, and was operated upon on October 19th. Mr. Cotterill reflected a large flap of bone, with the scalp adherent to it. A large portion of the motor area of the brain was thus exposed. The bone and dura were found to be quite natural. The surface of the brain did not look absolutely normal; there was, however, no increased intra-cranial pressure; some white lines were seen along the vessels which had a more glistening appearance than normal; the appearances seemed to indicate a chronic inflammatory process. No alteration in the consistency of the brain could be detected, and there was nothing which could be definitely identified as a tumour. The exposed area of brain was stimulated with the faradic current, but no response (spasms) resulted; it was thought that this was perhaps due to the fact that the chloroform narcosis was too powerful. However, be that as it may, no contractions were produced by the faradic current. A fine trocar was then inserted into the brain-tissue in various directions, because of the possibility of there being an abscess; nothing but clear cerebro-spinal fluid was withdrawn; the trocar had evidently penetrated into the ventricles. It was thereupon decided to do nothing more. The flap of bone was replaced after the dura had been very carefully sewn together with silk stitches. The scalp wound was sewn up with horsehair. The whole operation was, of course, carried out under the strictest antiseptic precautions. That was on October 19th. In the course of a fortnight the patient was sufficiently well to be sent back to the medical wards; he was absolutely well so far as the effects of the operation were concerned. The whole wound was soundly healed and covered with collodion.

With regard to the effect of the operation on

the fits, almost immediately after the operation, the same night, he had a return of the spasms, and from this time onwards he continued to have the fits just as he had done before. When he came again under my care I once more put him upon iodide and bromide of potassium and chloral hydrate, but no improvement resulted. Some time, a month or six weeks, afterwards, as he was obviously getting worse, and the paralysis and emaciation were increasing, I urged him to have another operation, with the object of having the discharging portion of the brain excised. He refused a second operation, and went home on January 13th, having two or three days previously had a general epileptic fit, in which he became unconscious, bit his tongue, and so on. This was the only generalised epileptic fit which occurred during the whole time that he was under my observation or prior to that. I saw his death announced in the paper a few days later, and I immediately wired to his doctor asking him to make a post-mortem examination. I heard from the doctor subsequently that the patient had had a second general epileptic fit, and had died in it. Fortunately, the doctor not only obtained a post-mortem examination, but he sent me both the brain and the skull-cap. The skull-cap is an extremely interesting specimen. I have referred to the wound on the surface. The preparation illustrates in a most striking way the extraordinarily beautiful effects of antiseptic surgery. I do not know any preparation which could give any body a better idea of the wonderful results which may be obtained by antiseptic surgery than this. You see the large flap of bone in position, without any evidence of elevation or inflammation around it; it is exactly in its place, and the trephine holes are filled up with firm fibrous membrane. If you look carefully at the inner surface of the dura mater, beneath the flap of bone, you will see that it is perfectly smooth and most accurately united, while the silk stitches are still there; they look as fresh as if they had been put in to-day. The most beautiful sempstress, dealing with dead tissues, could not have joined the edges of the incision and put in stitches more accurately than the surgeon has done in this dura; they are absolutely in position and preserved perfectly, though the patient lived for three months after the opera-

tion. The preparation shows that in suitable cases, where asepsis is effectively carried out, that there is practically little or no danger in removing even such a large flap of bone as is shown here. But you must remember that in this case we had to do with an intra-cranial tumour in which there was no increase of intra-cranial pressure. That is a fact of the very greatest importance as regards the results of operation. In a considerable number of the cases of intra-cranial tumour which I have seen operated upon the results have been far from favourable. In some cases a leakage of cerebro-spinal fluid goes slowly on, the edges of the wound become sodden and corroded, as it were, and the patient dies exhausted and emaciated; in other cases cerebritis may develop, in spite of antiseptic measures; a hernia cerebri is very common; there may be a marked increase of paralysis, and sometimes death a few days after the operation in those cases in which the increased intra-cranial pressure is great; and, of course, the patient may die from shock. I will pass the skull-cap round, so that you may see the successful result from an operative point of view.

Let me return to the post-mortem findings. When we removed the dura from the brain it was slightly adherent by means of delicate adhesions over the motor area. I handed the brain to Dr. Muir, now Professor Muir, of Glasgow, one of our ablest pathologists, and he gave me a detailed written report, which I need not, however, read to you in full. That report showed that the middle part of the ascending frontal convolution, which had been fully exposed at the operation, was swollen, and that just below the fissure of Rolando, one inch and a half from the surface, between the ascending parietal convolution and the ascending frontal there was a well-marked gliomatous tumour, which had obviously made its way to the surface and partly infiltrated the superficial parts. I cannot say whether the infiltration of the superficial cortex was present at the time of the operation or not, but the oldest part of the tumour was one inch and a half from the surface, under the fissure of Rolando, between the ascending frontal and the ascending parietal convolutions. I take it that that is the reason why the presence of the tumour was not detected at the operation, because there is no doubt

that that part of the brain immediately superficial to the tumour was thoroughly exposed. The tumour consisted of spindle-cells, and Dr. Muir thinks it should be classed rather as a sarcoma than a glioma.

Such are the chief facts of the case. There are one or two points of interest in connection with the case which I may venture to emphasise. In the first place, it is a very good illustration of a case of cerebral tumour in which there are few, if any, general symptoms—no optic neuritis, no vomiting, practically no headache, no giddiness,—but in which there are well-marked localising symptoms. I repeat that it is important to remember that the absence of the general symptoms does not exclude tumour.

Another point in connection with the case, which is of great interest, is the very long continuance of the Jacksonian epilepsy. This patient had been having fits for five months before he came under my observation; he was under my care for three months, having fits the whole time; so that he must have had attacks of Jacksonian epilepsy more or less continuously for a period of seven to eight months. When he came into the infirmary, he was having, on an average, ten fits every hour: he must have had many thousands of fits altogether. One has seen cases before of a somewhat similar kind, but I have not personally met with any case in which the patient continued for eight months to have attacks of Jacksonian epilepsy unrelieved by any treatment and in which the fits were so frequent as in this case.

Another point of great practical importance is the difficulty of recognising at an operation whether the brain-tissue is gliomatous or not. This is not to be wondered at when it is remembered that it is often exceedingly difficult to determine the same question post mortem. I have often, at a post-mortem, had great difficulty in deciding whether there was a gliomatous infiltration of the brain or not. The only change may be that the affected (infiltrated) portion of brain-tissue has a more glistening appearance than normal; there may be no increased vascularity and no change in colour, nor yet any change in consistency, though this is less frequent. If you expose the surface of such a brain at the time of operation, when there is blood and other

fluid about, it is a most difficult thing to say whether it is gliomatous or not. One very important method of distinguishing such a gliomatous infiltration is, probably, the application of the faradic current. As in monkeys and dogs, so in men, the application of a very weak faradic current to the surface of the motor cortex produces localised spasms. I have had an opportunity of demonstrating these spasms in several cases of brain disease which I have had operated on, and I have been very much struck by the fact that it requires a very weak current to produce muscular contractions, and that the areas in which the different movements are represented are extremely sharply defined and differentiated. You apply two copper needles, which are blunt at the point, over a given area, and you get a certain spasm; and then you apply the needles perhaps only a quarter of an inch, or even an eighth of an inch, outside this area, and you get another definite form of spasm. I have seen in a trephine wound several areas capable of being mapped out in that way. In the motor cortex of the normal brain muscular spasms are produced by a very weak current, and they are very localised. It is exceedingly difficult to say why in this particular case no spasms were produced, unless we grant that the portion of brain which was faradised was infiltrated with gliomatous tissue and unable to be irritated. I suggest that this may perhaps be an important, and in some cases, short of microscopic examination of an excised part of the cortex, the only means of differentiating, during life, normal brain-tissue and brain-tissue infiltrated with a glioma.

If time permits I will mention another case of tumour in which, although there were marked changes of colour on the surface, the application of the faradic current was of material assistance in enabling us to define the exact limits of a tumour.

I am not prepared to say absolutely that in the particular case which I have brought before your notice, the cause of the non-appearance of faradic contraction was due to a gliomatous infiltration, because in another case of Jacksonian epilepsy which I had trephined two years ago, in which there was no marked paralysis, electrical irritation also failed to produce contractions. In that case various strengths of current were repeatedly applied over the exposed surface of

the motor cortex without producing any muscular contractions, and we allowed the patient to come almost out of the chloroform, so as to avoid any fallacy from the narcotic. In that case, too, there was nothing deeper down than the cortex which could have prevented the discharge being carried downwards, for there was no paralysis. We were much puzzled by this result. As the fits continued to recur after the first operation, a second operation had to be performed in that case, and a fresh area of cortex had to be exposed; but the result of electrical stimulation was the same—we failed to get any faradic contractions. I cannot say what the reason of that was; it is a point which, I think, requires, and which will, probably, repay further investigation. Nevertheless I am satisfied that in many cases the application of the faradic current to the exposed surface of the brain is not only an important means of locating the exact part of the brain with which one is dealing, but it shows whether that part of the brain is normal or not.

Another point that this case illustrates is the risk of death in a fit. Everybody who has had much experience of intra-cranial tumours knows that patients very often die in a general epileptic fit, and for that reason one must not put off operating too long.

I may repeat that this case illustrates in a very remarkable way the safety of surgical procedure, particularly in those cases in which there is no increase of intra-cranial pressure; but, as I have already pointed out, it is a very different thing in cases in which the intra-cranial pressure is greatly increased—there the risks of operation are not inconsiderable.

I have incidentally referred to another case, and, as I have a few minutes left, I will give you some details of that case, for it is an exceedingly important one. It is that of a patient whom I saw last year. He was a man *æt.* 35, who was brought to me by Dr. Fraser, of Berwick, on October 9th last year, suffering from occasional attacks of Jacksonian epilepsy, but without any other symptoms whatever except that there was a local tender spot about the foot area, and that, although eating and digesting well, he was considerably emaciated.

The history of the case was that some nine months before I saw him he had had a general

epileptic fit, for which there was no apparent cause, and which was thought to be due to toxæmia. The case seemed so interesting that Dr. Fraser published it in the 'Lancet' on April 29th, 1899. The first fit was followed at considerable intervals by two or three attacks of localised spasm affecting the left leg and the left arm. The patient consulted two leading neurologists in London, one of whom thought there was no tumour and the other thought there was a tumour. He came under my care early in October. He had had altogether some five or six attacks of Jacksonian epilepsy, the spasms beginning in the toes of the left foot; there was some paralysis of that foot; a well-marked aura of a sensory kind, which I need not describe, occurred before the spasms. There was no headache, no vomiting, no giddiness, and no optic neuritis. There was no history of syphilis or of head injury. He had been treated with iodide of potassium and mercury. My diagnosis was a discharging lesion in the position of the left leg centre; and, as in the case I have already detailed to you, I arrived at the conclusion, by the method of exclusion, that the lesion was probably a gliomatous tumour, probably of small size. As the patient had only been able to take small doses of iodide of potassium, I advised that the drug should be again given, if possible, in full doses. The patient came to Edinburgh at the beginning of November for more careful observation and further treatment. I at once put him upon large doses of iodide of potassium; he took the drug quite well. He went on taking the iodide for three weeks, and expressed himself as feeling quite well, though he did not put on flesh and did not gain power in the weak foot. About the middle of November he had an attack, or rather a series of attacks, of Jacksonian epilepsy, and I had an opportunity of observing them. They began with a distinct aura—a painful feeling in the left thigh, passing down the leg; the spasms commenced in the toes of the left foot, then passed up the leg; they very rarely affected the left arm, and never the face. There was evidently a discharging lesion of the left foot-centre, for the spasms always commenced in the left foot. As he had had big doses of iodide of potassium and they had not produced the least effect, it was

decided, after consultation with Mr. Cotterill and Dr. Fraser, to operate. A large portion of bone over the leg-centre on the right side was removed by Mr. Cotterill. There was no marked increase of intra-cranial pressure. The tissue exposed did not look normal; its consistency was increased, and it was more purple than normal. The faradic current applied over the purple area did not produce any contractions, but at its lower margin produced very marked contractions in the left leg. The lesion turned out to be a tumour, and not a tumour of small size, but one of very large dimensions. It was almost as big as my fist. It was a sarcoma growing from the dura. After it was shelled out from its capsule it left an enormous hole. For some hours the patient was extremely collapsed and ill, but recovered quickly under appropriate treatment. The operation was performed on December 6th. On December 15th the stitches were removed; on December 21st the patient was able to get out of bed; on January 1st he went out for a drive; and on January 4th, less than a month after the operation, he went home. I saw him on June 8th of this year, more than six months after the operation, and, with the exception of very slight weakness in the left foot, he was absolutely well. The case will, of course, be reported in full by Mr. Cotterill and myself. I merely now mention the leading features of it: for it is another case of very great interest, in which there were no general symptoms, but well-marked local symptoms; and it shows that one may have a very large intra-cranial tumour without any marked increase of intra-cranial pressure, and without any of the general symptoms to which increased intra-cranial pressure gives rise. It is also one of the comparatively few cases of successful operation for intra-cranial tumour which have been recorded.

Different authorities hold different opinions as to the proportion of cases of intra-cranial tumour which are suitable for operation, but my own opinion is that it is only in a very small proportion of cases that an intra-cranial tumour can be successfully removed by the surgeon. In cases of intra-cranial tumour we operate for two reasons, one is to remove the tumour, and the other to relieve the symptoms. If you take the cases in which an operation is performed with the object of removing the tumour,

I am satisfied that the proportion of successful results is smaller than is usually supposed. The case I have related to you last is one of the cases in which a brilliant result is obtained; but it is the only case of tumour I have personally met with in which the operation has been successful. Although I have had a considerable number of cases of brain tumour operated upon, some with the object of removing the tumour, and others as a palliative measure, I have had no other case in which an intra-cranial tumour has been successfully removed. Authorities say that eight per cent. of brain tumours are capable of being successfully removed. I do not think the percentage is anything like that figure; I doubt, as the result both of my clinical and pathological experience, if it is as high as five per cent. But of course one man's experience is very limited, and it does not do to draw dogmatic conclusions from one's own personal observations. All I can do is to give you the result of my own experience, clinical, post-mortem, and operative, and it is that there are comparatively few cases of intra-cranial tumour which can be successfully removed by operation. In most of the cases in which a brain tumour can be successfully removed, the tumour is situated in the motor area, where it can be easily reached; and in a large proportion of the most successful cases the tumour has been enclosed in a capsule. It is in these cases that the most brilliant results are obtained.

There are, of course, many other points connected with the large and important subject of intra-cranial tumour which I should have liked to have brought before your notice. I have put in as much as I could in the time at my disposal, and I hope I have, at all events, interested you.

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CYCLIC ALBUMINURIA.

BY

G. A. SUTHERLAND, M.D.,

Physician to Paddington Green Children's Hospital and to the City Orthopædic Hospital; Assistant Physician, North-West London Hospital.

(Continued from p. 384.)

ÆTIOLOGY AND PATHOLOGY.

Various views have been expressed as to the ætiology and pathology of this affection. So far as I am aware no necropsy has yet been performed on a subject of this condition, consequently theories are propounded with a freedom which might be considerably curtailed if we possessed definite knowledge as to the condition of the kidneys after death. In his original paper on the albuminuria of adolescents, Dr. Moxon expressed the opinion that crystals of oxalate of lime were so frequently present that they might have induced active renal irritation. Mr. Clement Lucas, from personal knowledge of Dr. Moxon's opinion, has stated that he laid stress on masturbation as a cause. This opinion is supported by Mr. Lucas himself, who finds an associated condition of late rickets present, also traceable to the same cause. Dr. Dickinson thinks that most of these cases have this origin, but that others occur in which the suspicion does not present itself. He says the condition is not one of nephritis, but the kidneys may become congested in connection with neighbouring organs. Dr. Tyson suggests that the defect may be not in the kidney, but in the composition of the blood, owing to mal-assimilation. Dr. Ostwald holds that perverted metabolism renders the walls of the blood-vessels permeable to albumin, and that consequently albumin finds its way into the urine when the renal vessels are under unfavourable hydrostatic conditions, *i. e.* when the body is in an upright position. Dr. George Johnson stated that the majority of cases could be traced back to a more or less remote attack of acute nephritis. In his original papers on "Cyclic Albuminuria" Dr. Pavy described the characteristic features of "albuminuria in adoles-

cents"—the cyclic character under normal conditions of life, the disappearance under prolonged rest,—but did not offer any explanation of the condition. He noticed analogous phenomena in the case of persons subject to the phosphatic diathesis, in whom phosphates appear in the urine in cyclic fashion. Sir William Broadbent says that cyclic albuminuria is a circulatory phenomenon due to the fact that the patient has assumed the erect posture. Professor Heubner has observed that cyclic albuminuria may or may not be preceded by acute nephritis, but even in the former case the disappearance of tube-casts from the urine is opposed to the view that the nephritis is persistent. He says the clinical phenomena are suggestive of a general pathological condition, associated with a special weakness of the renal vessels and albuminuria. Drs. Marie, Teissier, and Lacour have all observed that cyclic albuminuria is commonly found amongst the descendants of the gouty and arthritic, and hold that this association is more than accidental. Dr. Marie thinks the affection is quite distinct from nephritis, and that it is a functional albuminuria probably dependent on disturbance of the sympathetic system, of which some other signs are usually present. Dr. Ralfe found a marked similarity between functional albuminuria and hæmoglobinuria, and that both were sometimes present simultaneously in the same individual. Both of those conditions he traced to increased hæmolytic action, and he suggested that this was due to an increased irritability of the vaso-motor reflex centre, which produced increased hæmolysis in the liver. Dr. Herbert Hawkins accepts the view that the vaso-motor mechanism regulating the blood-flow into and out of the glomeruli of the kidney is at fault, and not the kidney itself. The activity and integrity of the glomerular epithelium are disturbed by these variations in the blood-pressure, and albumin appears in the urine owing to the failure of the glomeruli to perform their selective function. Mr. H. L. Barnard, in discussing the condition of weak venous circulation from insufficient action of the skeletal muscles, resulting in overfilling of the abdominal vessels and cerebral anæmia, states that this is one of the less recognised causes of the albuminuria of adolescence.

It is probable that some of the above opinions have been based on the study of cases which do

not come into the class under discussion. This, during recent years, has been more sharply defined. Taking these views as a whole, we find that some writers regard cyclic albuminuria as indicative of organic renal disease, past, present, or future; others regard it as a physiological condition; while others think it a symptom of a general pathological state.

The evidence bearing on the view that cyclic albuminuria is dependent on previous nephritis has already been discussed. Careful examination and prolonged observation of these cases have not led to the discovery of any proof of the existence of organic renal disease, or of the development of signs of organic renal disease. On the contrary, a mass of evidence has been accumulated which goes far to disprove the association of cyclic albuminuria with any known form of Bright's disease. The mixed cases in which a condition of cyclic albuminuria is associated with nephritis have already been referred to a different category.

The view that the condition is a physiological one probably arose from the fact that the albuminuria was, as a rule, accidentally discovered; that no symptoms of Bright's disease or any other disease were present; and that the patients were described as being in the enjoyment of good health. Further investigation has revealed allied symptoms, although not those of Bright's disease. Heubner, Marie, and others have gone into these very fully; and, as regards my own cases, they were all patients who presented themselves with various symptoms. Most certainly they were not in good health, nor can they be said to have attained to a state of normal health while they have been under observation, and the albuminuria has persisted. Mr. Barnard's suggestion that the condition was dependent on sluggish abdominal circulation with venous congestion was very plausible, and I hopefully applied the remedies he outlined. One of the well-marked cases was selected, and this patient was submitted to vigorous abdominal massage, with passive exercises before rising, a tight flannel bandage being also applied round the abdomen. These measures had absolutely no effect on the incidence of the albuminuria, even when supplemented with treatment by strychnine, digitalin, and later trinitrine. I can only conclude that he was referring to another variety of albuminuria than that under discussion. Some writers hold the view that the

increased pressure in the renal blood-vessels, consequent on the assumption of the erect posture, is a sufficient explanation of the albuminuria. If man had only been content, like other animals, to go on all-fours, then cyclic albuminuria would never have arisen to puzzle physicians. This doctrine has the merit of simplicity, but it scarcely overcomes all difficulties. If it were the case, we should confidently expect the condition to be a common, if not universal one, instead of its being one of comparative rarity. Not only would albuminuria be present, but there ought to be also an increase in the amount of urine after exercise, whereas the opposite is the case. The total amount of urine passed by these patients is frequently below normal, and it is not uncommon to find that of that amount the greater part is passed after the night's rest; while during the day, when the extra pressure is present, the albuminous urine is scanty. Such a view is also directly opposed to the teaching of physiologists, who assert that the renal blood-vessels are under the control of a special nervous supply, by means of which they are rendered independent of the state of the circulation in other parts of the body.

The third view, that there is a general pathological condition present to which the various symptoms may be ascribed, has received a good deal of support on the Continent from Heubner, Marie, and others. The study of my own cases has led me to a similar conclusion. The subjects of this affection may be apparently healthy, but they are not really so. The underlying condition would appear to be a peculiar instability of the nervous system, which is brought into prominence at a period of active development, sometimes as the result of an acute illness, and sometimes without any apparent cause. It is manifested in the general condition of these patients, who are nervous, excitable, emotional, prone to headaches, tiredness, lassitude, broken sleep, night terrors, etc. It is seen in the tendency to local disturbances of function; in the heart (fainting attacks, tachycardia, irregular action); in the peripheral circulation (weak and irregular pulse, flushing, sweating); in the alimentary system (gastric, hepatic, and intestinal disorders); in the urinary system (albuminuria, oliguria, with high specific gravity and excess of solids); and in the sexual excitability which may lead to masturbation. Although none

of these symptoms would appear to be of much importance taken by itself, when they occur together, as they usually do in patients seeking medical advice, one is compelled to recognise the existence of a special affection. Regarded as a whole, this affection, as already pointed out, has many points in common with that known as the uric acid diathesis, and both may have a common origin in a gouty or rheumatic parentage. We are still in a state of uncertainty as to the exact pathology of the uric acid diathesis, and a conflict of opinion exists as to whether the primary condition is one of faulty metabolism with secondary disturbances of nervous function, or of nervous disorder with consequent disturbances in metabolism. The uric acid diathesis is, in all probability, the result of a condition of toxæmia, with uric acid or some other product of imperfect digestion circulating in the blood, and producing the symptoms of general malaise and of nervous disturbance. Cyclic albuminuria probably also represents a condition of toxæmia similar to, or identical with, that of the uric acid diathesis. Further evidence in support of this view is furnished by the association, already referred to, of cyclic albuminuria and Graves's disease. In the latter affection the signs of nervous disorder are very marked, and the condition is now usually regarded as one of toxæmia, the poison in this case being, according to some authorities, an excessive amount of thyroid secretion or an altered thyroid secretion. It does not seem at all improbable that, given a neurotic predisposition, similar disturbances in the nervous centres may be produced by different poisons in the blood.

The immediate cause of the postural albuminuria is difficult to explain. It was very natural to assume, as some have done, that the excessive amount of uric acid or oxalates in the urine had produced albuminuria by direct irritation of the kidneys. The presence of these substances in excess is a marked feature in persons suffering from cyclic albuminuria, and may be responsible for some attacks of abdominal pain, for frequency of micturition, or even for intermittent albuminuria. The conditions of occurrence of cyclic albuminuria are, however, entirely different. The rapid appearance and disappearance of the albuminuria under changes of position, and the persistence of these conditions for months or years, long after any gravel can be found in the urine, will not be found in

cases of renal irritation from uric acid or oxalates. As regards the view that masturbation was the cause of the albuminuria, which receives support from Moxon, Dickinson, and Clement Lucas, I have found that this form of sexual neurosis was present in a considerable proportion of my cases. While confirming their observations as to the existence of this neurosis, I cannot accept their conclusion that it is the cause of the albuminuria. Not only does cyclic albuminuria occur quite apart from this neurosis, as proved by cases carefully observed to determine this point, but the rapid cessation of albuminuria when the patient is at rest, and its continued absence while he remains lying down, are quite inconsistent with such a view of the ætiology. There can be little doubt that the albumin is of renal origin, as there is no evidence of catarrh or other affection of the urinary passages sufficient to explain its presence, and far less the conditions under which it occurs. In the present state of our ignorance of the renal changes in this affection no explanation of the albuminuria can be considered as final, but the one which appears most probable is that which refers the condition to a vaso-motor disturbance of the renal blood-vessels, dependent, in all probability, on some central vaso-motor instability. The disturbance in the renal circulation, which appears to be accompanied by lowered rather than increased blood-pressure, produces such changes in the vessels of the glomeruli that the epithelium is temporarily affected, its protective function diminished, and albumin transudes from the blood. After a longer or shorter period the glomerular circulation is restored to normal, the epithelium recovers its power, and albumin ceases to appear in the urine.

DIAGNOSIS.

Dr. Pavy states that in his cases the albuminuria was discovered accidentally, and this is not an uncommon experience, especially in connection with medical examinations for life insurance. In such cases it may be that the accompanying signs of the condition had subsided, or had never been of a marked character. For although amongst my earlier cases the urine was examined rather as a routine proceeding in the absence of any definite signs of visceral disease, the patients had all presented themselves in a state of ill-health, seeking

relief from symptoms which were undoubtedly associated with the cyclic albuminuria. We must note at the outset that the patient does not come complaining of any one leading symptom—such as coughing in pulmonary affections, and does not manifest any distinctive local change, such as a swollen and tender joint in rheumatic fever. The patient has had no indication of any urinary trouble, and the examining physician will not gather from the history any indication of an affection of the urinary system unless he has in his mind a clear picture of the symptomatology. It is necessary, therefore, to consider what are the symptoms usually complained of, what are the signs discoverable on objective examination, and more especially, how is a condition of cyclic albuminuria to be determined.

A case will usually present itself as follows :

A boy or girl of from ten to fifteen years of age complains of headache, loss of appetite, attacks of abdominal pain, and aching in the limbs and back. He has been unusually listless, easily tired, and disinclined for games or active exercise of any kind. He has been irritable, excitable, easily frightened, easily depressed by a sharp word, apt to find his school lessons a burden and school itself a trial. His nights have been broken by alarming dreams, by talking in his sleep, or by his getting out of bed. The history so far shows that we have evidences of nervous disturbance. On examination one finds usually a thin child, with signs of sluggish circulation in the blue and cold hands, and evidences of vaso-motor instability in the flushes or erythemata which appear on the face or body during excitement. The eyes may be rather puffy, and are often surrounded by dark rings ; the cardiac action may be rapid and irregular ; the pulse is usually small and of low tension. Examination of the internal organs reveals no signs of organic disease, and one may then suspect some renal condition. Here arises one of the difficulties of diagnosis. Supposing that the examination is being made in the afternoon, we may find the urine normal in every respect or containing a mere trace of albumin,—that is to say, the albuminous cycle for the day may be over, or nearly over. The urine may be regarded as normal and further examination considered unnecessary, or the patient may be ordered to bed for further examination, a suspicion of Bright's disease being present

in the physician's mind. A common sequence of events is that on the following morning, a specimen of urine passed while the patient is in bed is found to be free from any trace of albumin, and no other sign of Bright's disease being discovered, the suspicion of renal disease is dismissed, and some other diagnosis is made. As a matter of fact the disappearance of albumin from the urine after rest in bed is really a most important diagnostic sign, and should suggest at once the advisability of testing carefully for cyclic albuminuria. These tests, if thoroughly carried out, are somewhat tedious, but are well worthy of adoption in every case. The patient should be kept lying down in bed for twenty-four hours, and every specimen of urine passed during that time should be examined. On the following morning he should be allowed to rise and go about in the ordinary way during the day, every specimen of urine during this day and the following night being carefully examined. A condition of cyclic albuminuria is determined if it be found that after going to bed the albuminuria soon ceases; that during the whole period of rest no albumin is present in the urine; that after rising albumin appears in the urine, in definite amount, once or oftener during the day, but as a rule tending to diminish or disappear towards the end of the day; and that albumin is again entirely absent some hours after the patient has retired to rest. If in addition the urine is normal in other respects, and more especially if no tube-casts are detected after centrifuging, then a diagnosis of pure cyclic albuminuria made be made. In cases where such a complete investigation is not possible, an examination of the urine passed on rising, at mid-day, and on retiring to rest, will usually supply sufficient evidence to allow of a correct diagnosis being made.

In some respects the signs present in cyclic albuminuria resemble those met with in the various forms of nephritis, just as in other nervous affections functional disturbance often simulates organic disease. The leading similarity is the existence of albuminuria, which may be associated with oliguria. A certain amount of œdema, chiefly marked in the eyelids and the prætibial regions, may also be present. Severe prostrating headaches, accompanied with vomiting, and occurring sometimes in the morning, are not infrequent. Pigmentation of the skin has been already men-

tioned, and this, according to Dr. Leonard Guthrie, is also "a most important diagnostic sign of interstitial nephritis in both old and young, although not existing invariably." The history given by the patients of general malaise, languor, and inability to work, suggests at first the possibility of renal disease. But while these symptoms are similar to, they are not identical with those of Bright's disease. They are slighter, more fleeting, more inconstant; they do not bear a proper relation to each other as regards severity; they do not correspond with any one clinical type of Bright's disease, but seem to touch all types in one or more particulars; and under prolonged observation they do not seem to develop into any recognised form of renal disease.

The differential diagnosis between cyclic albuminuria and Bright's disease is based on both positive and negative evidence. The cyclic condition of albuminuria with complete disappearance of the albumin under rest, is positive evidence against a diagnosis of nephritis. Negative evidence is supplied by the absence of cardio-vascular changes, of retinal changes, of tube-casts other than hyaline, of uræmic attacks, and of progressive ill-health in spite of treatment.

Cyclic albuminuria must also be distinguished from the other varieties of functional albuminuria.

1. *Diet albuminuria*.—As regards the recorded cases of "after breakfast" albuminuria, it is extremely probable that many of these were really examples of cyclic albuminuria, in which the breakfast was erroneously assumed to be the cause of the albuminuria. This point ought always to be carefully tested—(a) by keeping the patient in bed for breakfast and for some hours after it, and then examining as to whether albuminuria has appeared in the urine; and (b) by getting the patient out of bed in the morning, giving him a couple of hours' exercise, and then examining the urine before any food has been taken. These tests will usually suffice to decide the question. The occurrence of a slight albuminuria after a meal, or after certain articles of diet, would appear to be an idiosyncrasy in some individuals, the significance of which is probably trifling.

2. *Exertion albuminuria*.—This, as the name denotes, is the result of prolonged muscular effort, and is always preceded by it. This form is therefore easily distinguished from cyclic albuminuria,

where the erect posture without any unusual muscular exertion is sufficient to induce the albuminuria. Exertion albuminuria is usually accompanied by the presence of casts in the urine, and indicates a disturbance of the renal circulation from over-pressure which is rather pathological than functional.

In addition to the well-marked cases of cyclic albuminuria under discussion, there are others in which the amount of albumin is small, the general symptoms are slight, and consequently the diagnosis presents considerable difficulty. They are by no means unimportant, because not infrequently they represent the initial stage of what may later develop into pronounced cyclic albuminuria with all the accompanying phenomena. The albuminuria is not only trifling in amount, but is often present one day and absent the next under similar conditions of life. Signs of nervous instability, of the same nature as those described but of a less marked character, are found to be present. Some evidences of vaso-motor instability will usually be detected on examination. These symptoms I have frequently met with amongst other members of a family in which one member was suffering from definite cyclic albuminuria. They may be regarded as borderland cases, which may develop into a condition of real cyclic albuminuria, or may terminate in recovery without the supervention of any marked symptoms.

A CASE SHOWN

AT A

THURSDAY CONSULTATION AT ST. BARTHOLOMEW'S HOSPITAL.

Hard Chancre within the Mouth.

MR. LOCKWOOD said he wished Mr. Marsh to look at the patient before them. She had been in the hospital a day or two, and there was some doubt as to the nature of her disease. The history was as follows:—She was fifteen years of age, and her occupation that of general servant. Two months ago she noticed there was something wrong in the

inside of her cheek, and felt a lump there. Apparently it had not been attended by very much pain. A fortnight ago she bit this lump and it began to bleed. At the present time she had inside her cheek an oval ulcer as large as a sixpence. It has a very hard base, perhaps a quarter of an inch thick, and the edges were raised above the level of the mucous membrane of the cheek. A probe was applied without apparently producing any pain. No surrounding inflammation could be detected, but there was a little slough in the centre, which had been attributed to the fact that she bit it. In the neck on the same side the lymphatic glands were a little hard and slightly tender. Six days ago, however, they were larger than at present. She was a little anæmic. Mr. Lockwood said he had examined her body, but could not discover any roseola, and she had no condylomata or superficial ulceration in her throat, but the fauces were reddened. There was an obscure history of a rash upon her legs some time ago, but it did not seem to have any bearing upon the present matter. To his mind the lesion looked more like a syphilitic sore than anything else, and he thought the anæmia and slight enlargement of the glands pointed in the same direction. For the same reason he would expect to see a little roseola before long; of course there were several other possibilities. He had already put her upon a little grey powder, of which she had had a small dose twice a day for three days, and already he could not help thinking that the sore looked cleaner and better; of course it might be tuberculous, and he believed the word actinomycosis had been used in connection with it. Still he saw no reason to regard it as either tuberculous or actinomycotic. The only point which would make him a little doubtful about the diagnosis was that she was supposed to have had it for two months, but, nevertheless, had not yet developed any roseola.

MR. MARSH said it was a very interesting case. He supposed the patient's age controlled the opinion formed. If she had been an older person epithelioma was not at all an impossible diagnosis, and he supposed one could not absolutely exclude the idea that it might be one of those very curious cases of sarcoma which had ulcerated after being injured. However, in the present case he would put these speculations out of his own mind; for he felt pretty clearly that this was a case of primary syphilitic sore, and he thought the history pointed in that direction, and particularly as she had not got very much enlargement of the glands since she had been taking the grey powder.

[*Note of case subsequent to consultation.*—The sore quickly healed under the influence of mercury, and the enlarged lymphatic glands grew smaller.]

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SOME POINTS

IN THE

DIAGNOSIS AND TREATMENT OF UTERINE FIBROIDS.*

BY

J. BLAND-SUTTON, F.R.C.S.

SURGICAL aggression in obstetric territory has brought many new things to light. To my mind one of the most astonishing facts thus revealed is the great uncertainty of clinical methods in determining the nature of pelvic tumours. I propose in this communication to draw attention to some examples of erroneous clinical judgments in regard to uterine fibroids, which have been exposed in the surgical treatment of these tumours, and it is certain that this method of radical treatment has led to increased accuracy in our knowledge of their pathology.

There is no tumour in the human body which in so many instances can be so surely recognised clinically as the uterine fibroid, yet this occasional certainty in diagnosis is in itself a source of danger when the question of operative interference is under consideration. The recognition of an uncomplicated fibroid is usually regarded as a very simple clinical exercise, nevertheless the conditions with which such a tumour has been confounded make a very long list and include ovarian cysts and solid tumours, pyosalpinx, gravid tubes in all stages, including foetuses of the full period sequestered in the mesometrium, normal uterine pregnancy, cancer of the body of the uterus, ecchinococcus colonies of the uterus, tumours of the kidney, and displaced spleens. If errors arise when the tumours are single, how much more must be the chances of false diagnosis when two or more pathological swellings co-exist in the pelvis, and especially if these should be associated with a normal or an abnormal pregnancy.

* An address delivered to the Pathological Society of Reading, October 11th.

I am sure that few to-day scarcely realise that twenty-five years ago it occasionally happened that even after a tumour had been removed from the pelvis, surgeons would wrangle as to whether it was uterine or ovarian; and the accounts of these remarks made on specimens exhibited at the London medical societies will be found very amusing in relation to this point, to any one who takes the trouble to hunt them out in the current literature of that period.

The chief signs on which a diagnosis of a "fibroid" is based are these:

The existence of a tumour which may be, and usually is, incorporated with the uterus. The tumour may be so large as to reach as high as the diaphragm, or no bigger than a potato of average size. The enlarged uterus may be of a smooth contour or irregular, and tuberosc.

Age is an important factor, for fibroids are rarely recognisable and troublesome before the twenty-fifth year, and there is every reason to believe that they arise only during the menstrual period of a woman's life.

In many cases fibroids, especially those which encroach on the endometrium, give rise to excessive and prolonged menstruation. In some instances the bleeding may be so free that the women die.

As a rule, a fibroid large enough to be troublesome adds to the length of the uterine cavity, and in a certain proportion of instances yields a distinct hum to auscultation.

Any tumour of pelvic origin which furnishes two or more of these signs, will often successfully simulate a fibroid; on the other hand, a fibroid which fails to furnish all these signs causes doubt in diagnosis. It is common experience that it is impossible sometimes to distinguish between a very large fibroid and an ovarian cyst, especially when the fibroid does not disturb menstruation and yields no hum on auscultation.

In 1899, I saw at the Cottage Hospital at Ealing, a woman about forty years of age with a large tumour filling and distending the belly uniformly; it was smooth in contour, dumb to auscultation, and had grown slowly for five years: there was no fluctuation. The uterine cavity was of normal length and menstruation occurred regularly and normally. At the operation this proved to be a huge globular fibroid (weighing thirty-six

pounds), sessile on the fundus of a uterus, which in size was proportionate to the age and stature of the woman. It was successfully removed, but before the operation all who examined the patient believed the mass to be in all probability a solid ovarian tumour.

It used to be a fairly frequent event for a surgeon to perform *cœliotomy* with the intention of removing a suspected ovarian cyst, but, on finding a fibroid, to close the wound and leave the tumour. I have on several occasions subsequently had charge of such patients and successfully removed their tumours. On the other hand, ovarian and parovarian cysts very frequently simulate fibroids. Three years ago a cook of forty years of age was seen at the Samaritan Hospital, and Mr. Meredith, who examined her, came to the opinion that she had a large fibroid: the case being urgent, and as the hospital was closing for the annual cleaning, she applied to St. Thomas's Hospital. The gynaecologist at this institution told her that she had a fibroid. Eventually she came under Dr. Fenton's care at the Chelsea Hospital for Women, and he confirmed the diagnosis and transferred her to my care; after careful examination I regarded the tumour as a fibroid, but at the operation I found bilateral ovarian cysts, one, as big as a cocoanut, impacted in the pelvis. In this case four individuals thoroughly accustomed to examine pelvic tumours were in error; fortunately, in this case it did not interfere with the appropriate treatment. It is, however, a much graver matter when a pelvic tumour is considered to be a fibroid of the uterus, and because it is a fibroid operative treatment need not be entertained; subsequently serious complications arise and the supposed fibroid turns out to be a suppurating dermoid, or something equally obnoxious and inimical to life. The following case illustrates the ill-effects of such erroneous diagnosis.

A married woman, *æt.* 40, suffered from a pelvic tumour which extended well into the right iliac fossa; she was under the care of an experienced gynaecologist, who regarded the tumour as a fibroid of the uterus, an opinion confirmed by an eminent obstetric physician. An inactive mode of treatment was pursued for several years, then bladder symptoms became so urgent and distressing that it became a pressing necessity

to attempt the removal of the tumour. A large dermoid filled with hair, teeth, macerated fragments of bone, phosphatic masses, and horribly offensive pus was found communicating with the bladder. The operation succeeded, and the patient completely and quickly recovered, but what a lot of misery and suffering this poor lady endured because of an erroneous diagnosis.

Two very important signs will, when present, enable us, in a large number of instances, to distinguish between an ovarian cyst and a fibroid:—An ovarian tumour yields no sound to auscultation and very rarely causes metrorrhagia. I have seen two instances in which irregular losses of blood from the uterus were associated with an ovarian tumour; both patients were twenty-eight years of age and the tumours had twisted their pedicles.

In the case of an ovarian cyst with a long pedicle, and a single pedunculated subserous fibroid, the physical signs are so similar that the distinction is very often mere guess-work. This is also true of many cases where an impacted fibroid softens to such a degree that it fluctuates on manipulation. Intuition, the outcome of long experience, will sometimes embolden men to venture on a differential diagnosis, but operation-records indicate that in more than half the cases the inferences are very erroneous. This is not a matter of any serious moment so long as it is not allowed to bar necessary surgical intervention.

Still confining our attention to the uncertainty of the differential diagnosis of simple uncomplicated tumours of the uterus and ovary, it is obvious that if there be so much difficulty in determining in which of these two organs a given tumour arises, how much greater must be the liability to error when attempts are made to decide the nature of the tumour.

Altormyan, of Aleppo, has related the facts concerning a woman of thirty-five years of age, who had a rounded mobile tumour of the size of a head which was thought to be ovarian in origin, but at the operation it was found to be an ecchinococcus colony in the fundus of the uterus. An even more striking case has been reported from Martin's Klinik, in Berlin. Coeliotomy was performed for the removal of two pedunculated tumours attached to the fundus of the uterus and suspected to be subserous fibroids. On lift-

ing them out of the belly they burst in the operator's hands. They were ecchinococcus cysts the size of a fist.

In order to more strongly emphasise the occasional difficulties which are encountered in the diagnosis of uterine tumours it may be mentioned that a wandering spleen is very apt to drop into the pelvis. Sir Spencer Wells performed coeliotomy in a lady, expecting to remove a uterine myoma; in the course of the operation a large purple-coloured mass was exposed; on manipulating it, his hand suddenly broke into a large, soft bleeding organ which proved to be an enlarged spleen lying in contact with the uterus.

Varneck had a stranger experience, for he operated on a woman and with great difficulty succeeded in removing piecemeal what he regarded as a firmly adherent uterine myoma. When the fragments were examined in the laboratory it was discovered that the supposed myoma was an enlarged, displaced, and adherent spleen.

On one occasion I performed coeliotomy on a woman for what I regarded as a wandering spleen. On exposing the tumour, a mass of the shape and colour of a large spleen was seen lying on the uterus. It proved to be a subserous myoma with a slender pedicle. It was not until I had exposed the spleen that I succeeded in convincing the onlookers that the mass was really a tumour and not the spleen.

On several occasions in performing splenectomy for "wandering" spleen, I have found this viscus in the pelvis in contact with, and displacing the uterus.

It must also be remembered that bias often counts for much in erroneous diagnosis. McGraw has recorded a good instance, in which a woman in the fifth month of pregnancy complained of severe pain in the belly, which led to the detection of a "lump" in the left lumbar region. The case was seen by a gynaecologist and a surgeon in consultation and regarded as a renal tumour. Coeliotomy was performed and the lump proved to be a sessile subserous fibroid weighing four pounds. It was successfully removed and the pregnancy went to term and ended happily.

A woman under my own care, four months pregnant, complained of pain due to a lump in the left iliac fossa. This was thought to be a pyosalpinx by one gynaecologist and regarded

as an ovarian cyst by another; I regarded it as a sessile fibroid, and so it proved at the operation, when I was able to remove it, and the patient recovered without disturbance. Successful abdominal enucleations of this kind are additional evidence of the tolerance which the pregnant stances, and which has excited the admiration of stances and which has excited the admiration of many observers and received its due "mede of praise."

Of the various signs on which reliance is placed in the diagnosis of "fibroids" the two most important are profuse menstruation and often metrorrhagia associated with a tumour incorporated with the uterus. Any condition which can produce a combination of these two signs usually renders diagnosis as uncertain as navigation in a fog. This combination is very common in diseases of the Fallopian tubes and especially in tubal pregnancy, and it comes to pass that of all pelvic swellings liable to be mistaken for fibroids, pyosalpinx, gravid tubes, and mesometric pregnancy hold the first place. Quite a large number of records may be gathered from the literature of the last ten years, where experienced men like Thomas Keith, Angus Macdonald, Duncan, and Herman, among others, have performed cœliotomy for the purpose of removing a suspected fibroid and have found a sequestered foetus instead, and in some of the cases the swelling has been excised and the operation completed, when a subsequent examination of the parts removed has disclosed a foetus. In spite of every care it is occasionally impossible to make an accurate diagnosis, as the following details prove:

In 1897, a woman æt. 38, came under my care for profuse metrorrhagia. The history and the physical signs indicated very strongly the presence of a submucous fibroid. The patient was anæsthetised and the cervical canal dilated; a large rounded mass could be felt as though a large sessile submucous myoma, embedded in the posterior wall of the uterus, was projecting into the uterine cavity. I consulted with two of my colleagues who were present at the examination, as to the advisability of enucleating it. After careful examination it was regarded as a safer measure to perform abdominal hysterectomy. The patient decided to defer so serious

a measure, but the bleeding continued to be so profuse during the succeeding three months that she sought relief. At the operation the supposed myoma proved to be a gravid left Fallopian tube containing a "mole" as large as a turkey's egg. The cœlomic ostium of the tube was completely occluded. The operation was followed by arrest of the metrorrhagia. The right ovary and tube were not removed and menstruation became subsequently normal in rhythm and in quantity. This case is also significant, as it favours the view that blood effused into a Fallopian tube with an occluded cœlomic ostium may escape by way of the uterus.

If difficulty and doubt arise in uncomplicated cases, how great must be the risks of error when two or more conditions co-exist. Uncomplicated pregnancy is, as a rule, diagnosed with certainty, but when pregnancy occurs in a uterus also occupied with fibroids it is not always a simple exercise. Some months ago a married woman came under my care, stating that she knew she had fibroids and was afraid also that conception had occurred. Her distress on this account was due to the fact that five years ago she had been in the same straits, and, though prematurely confined, had been so ill and ran such a narrow risk of dying that she wished to avoid the repetition of such a contingency if possible. On examination a fibroid was found to occupy the true pelvis, and a larger one the false pelvis on the left side: the uterus from the history and signs had probably been gravid two months. A colleague, who examined the patient, satisfied himself that if the pregnancy continued, it was quite certain that the foetus could not enter the world through the natural passage, and after very careful consideration of the facts she was advised to submit to hysterectomy. This was carried out with the preservation of one ovary; and the patient left the hospital convalescent, grateful, and happy in eighteen days. The uterus is shown in the illustration with the foetus in its place. If time permitted I could furnish quite a number of references where normal pregnancy, and retroflexion of the gravid uterus have been mistaken for fibroids and *vice versa*; and even when an operation was undertaken a retroflexed gravid uterus has been mistaken for a fibro-myoma.

Probably one of the most extraordinary

examples of diagnostic difficulties where fibroids were concerned, is the case recorded with great care and detail, by Cullingworth ('Trans. Obstet. Soc.,' vol. xl., p. 285), in which fibroids and tubal pregnancy co-existed. The physical signs so simulated retroversion of a gravid uterus that on September 4th deliberate but unsuccessful attempts were made to reduce the supposed displacement. On October 5th a further but equally ineffectual effort was made. Misgivings then arose as to the correctness of the diagnosis, and on October 21st coeliotomy was performed, and a large fibro-myomatous uterus, complicated with a gravid tube containing a foetus four and a half inches long, was removed. It is gratifying to add that the operation was followed by an excellent recovery.

tumour, which arose in the ovary and proved to be a solid sarcoma composed of oat-shaped cells. The issue of the operation was all that could be desired, but it is impossible to forecast the results of an unfortunate delay of three months in the removal of the tumour, on the future of the patient. This is an example of the great injury sometimes unconsciously inflicted on patients by an overweening confidence in our powers of diagnosis. It is also certain that women often endure unnecessary misery by our blind belief in the fact that "fibroids" are tumours capable of easy clinical recognition. Fortunately surgeons are now realising that with the employment of strict aseptic measures in pelvic surgery, uterine fibroids can be removed with a measure of success which has lately been astonishing, so that in



It is not my intention to attempt an exhaustive or systematic account of the differential diagnostic signs of uterine fibroids, but merely to show that in many cases it is very difficult and often impossible to decide between these common tumours and other very grave conditions, this being a matter of great importance. For instance, a woman æt. 25 came under my care with an abdominal tumour bigger than a football, stating that she had been in a hospital and the surgeon refused to remove it. This seemed curious, and my house surgeon communicated with him and received a letter to the effect that the tumour was a uterine myoma and did not require an operation. I did not agree with the diagnosis, acceded to the patient's request, and removed this very large

the future women with pelvic tumours of uncertain nature will be spared much suffering by the timely intervention of surgery.

THE indication for prescribing iron is so often only considered to be dismissed because of the many well-known digestive and assimilative difficulties attending its exhibition that medical men will welcome the appearance of a valuable ferruginous preparation by Allen and Hanbury, named Byno-Hæmoglobin, made from fresh blood and liquid malt. There is no doubt that Byno-Hæmoglobin will be used widely in the treatment of the many diseases requiring iron in a form easy of absorption, and at the same time agreeable to the palate.

A CLINICAL LECTURE
ON A CASE OF
**INTERSCAPULO-THORACIC AMPU-
TATION FOR PERIOSTEAL SAR-
COMA OF THE UPPER END
OF THE HUMERUS.**

BY
**G. JAMESON JOHNSTON, M.A., M.B.,
F.R.C.S.I.,**

Surgeon to the Royal City of Dublin Hospital.

GENTLEMEN,—The following case, in addition to possessing clinical interests of its own, illustrates very well some of the most important principles which underlie modern surgery. Maggie C—, a well-made, healthy-looking girl, æt. 18, was admitted to hospital on October 31st, 1899, under the care of my late colleague, Mr. Wheeler. The last message I had from him before his death was a request to take over this case. The girl complained of a swelling of her left shoulder which had gradually formed and was steadily growing during the past six months; for the first three or four months the growth was quite painless; however, recently, she began to suffer considerable pain of a dull character which was exaggerated by movement. The pain increased so much during her stay in hospital, previous to operation, as to necessitate hypodermic injections of morphia every night. Examination of the affected shoulder revealed a large swelling round the upper end of the humerus involving chiefly the soft parts on the anterior and outer aspects of the deltoid region, as was easily seen by looking vertically down on it. The skin over the swelling was not involved in the mass and appeared normal except that the veins were somewhat over-distended. The photograph here reproduced, kindly taken for me by my dresser, Mr. R. G. Tate, gives a better idea of the distribution of the enlargement than any description. Palpation indicated the presence of an inelastic, non-fluctuating, semi-solid mass. No enlarged glands could be felt in the axilla or neck. No crepitation could be elicited, "egg-shell" or other. Active movements of the joint were very painful and limited. Passive movements were much more free and caused comparatively little pain. There was a vague history of a fall on the

shoulder six months previous to the appearance of the tumour, but the injury was not of such a degree, however, as to suggest that a fracture had occurred.



Now with the above history and examination it was necessary to make a diagnosis. Taking the gradual onset, possible association with injury, the progressive and fairly rapid growth, the situation and consistence of the swelling, I came to the conclusion that the condition present was sarcoma of the upper end of the humerus. The asymmetry of the swelling as regards the bone, the rate of growth, and the absence of "egg-shell crackling" supported the idea that it was periosteal and not endosteal. The mobility of the joint and the absence of crepitation caused me to think the joint was not affected; this is usual in sarcoma until fairly late. In one of my specimens the upper four inches of the tibia is replaced by sarcomatous tissue, on which the articular cartilage, as it were, floats; the tumour has also grown up along the sides of the patellar ligament, but nowhere has it penetrated the joint. Though my mind was practically made up as to the diagnosis,

the unfavourable prognosis which this foreshadowed, and the remote possibility of tubercular arthritis, caused me to leave no method of physical examination known to me untried. I therefore inserted an aspirating needle and with some difficulty succeeded in drawing off about a drachm of venous-looking blood, which so far tended to confirm the diagnosis already made. I then had a Röntgen ray photograph taken by my friend, Mr. W. S. Haughton, who produced an excellent skiagram showing clearly a tumour surrounding the head and upper portion of the shaft of the humerus as far down as the deltoid ridge; the tumour substance was more opaque than the normal soft parts and the humerus could be distinctly traced through it; the surface of the bone, where surrounded by the tumour, seemed slightly eroded and faint indications of spicules of bone could be seen in the tumour itself; the shoulder joint was apparently intact. It was now evident the tumour was not a central sarcoma. Had it been so, microscopical examination would have revealed the structure, and in case this proved to be myeloid "sarcoma," the subsequent treatment would have been much less radical.

The next question was that of treatment. Having had negative results in three cases of sarcoma in which I tried the mixed erysipelas and *Bacillus prodigiosus* toxins treatment, and seeing the possibility of removing the growth by operation, I decided to do an interscapulo-thoracic amputation, disarticulation at the shoulder-joint being impracticable from insufficiency of healthy flaps.

On November 18th the patient, having been prepared in the usual way the evening before, was anæsthetised with ether, the operation area and its vicinity then thoroughly scrubbed with Calvert's surgeons' soap (containing pumice and phenol, and made at my suggestion by Messrs. Calvert, of Manchester), ether and biniodide of mercury solution, the patient drawn well to the edge of the table and the shoulders raised. A hypodermic injection of $\frac{1}{30}$ -grain of strychnine was given just before commencing to operate. I made an incision into the tumour and removed a small piece, which Dr. Littledale was good enough to examine for me and inform me in a few minutes that the growth was a mixed cell sarcoma. I then proceeded with the major operation, making an incision from the outer edge of the sterno-mastoid

origin along the clavicle to its outer end; the periosteum was incised and separated from the middle third of the clavicle, which was removed by means of a Gigli's wire saw, a most useful instrument for this class of work; the subclavius muscle and the various fasciæ were cut through, exposing the subclavian artery and vein; two silk ligatures were passed round each; the limb was raised and held in a vertical position for two or three minutes; the ligatures round the artery were tied and then those round the vein; both vessels were now severed between the ligatures; the limb was then lowered. Seeing the supra-scapular artery coursing across the wound I thought it wise to tie it at this stage; these were the only ligatures used during the whole operation. An incision was now made from the middle of the previous one vertically down to the lower edge of the pectoralis major where it gradually curved back to end in the mid-axillary line about four inches below the axilla; the pectoralis major and minor were cut through close to their origins, as the tumour encroached more on the anterior part of the axilla than behind; the brachial plexus was then quickly cut through with a very sharp scalpel just as it emerged from between the scalene muscles; the limb was then drawn across the chest by an assistant and the patient slightly rolled over on the sound side. A third incision was made commencing at the outer end of the clavicular incision and running almost vertically down the posterior aspect of the shoulder to meet the lower extremity of the anterior incision, the trapezius was cut through near its insertion, the omohyoid separated from the upper border and the levator anguli scapulæ, rhomboids and serratus magnus from the vertebral border of the scapula, the latissimus dorsi cut through and the limb removed. All bleeding points were stopped by pressure forceps. The blood lost did not exceed six ounces in addition to the small amount left in the limb. Every incision was made well inside the tumour mass and through tissue which showed no sign of contamination. The wound was closed in its entire length by intermittent silkworm-gut sutures, no difficulty being experienced in apposing the edges; no drainage material was used; dressings were applied, a nutrient enema containing brandy was administered, and the patient sent back to the ward. Without my being aware of it the time

taken to perform the operation was noted and I was subsequently informed this was forty minutes. The introduction of interrupted sutures naturally was tedious, but I think I get better apposition with this form of suture than with any other, and they are much more easily removed.

The patient suffered very little from shock. I saw her two hours after the operation and was surprised to find her so well, no pain, pulse 100 and fairly strong, temperature 97.8° F. She expressed a wish to be allowed to sit up and look about her. The evening of the operation her temperature was 99°, next morning 100°. Second evening after, 101°. As this did not fall on the third day I dressed the wound and found everything right except that there was some bagging at the lower end. I removed two stitches, which permitted about four ounces of altered blood to escape. The temperature fell to normal that evening and remained so. The wound healed *per primam* except for about an inch where the stitches had been removed. The patient was exhibited with the wound entirely healed on the



twentieth day after operation at the Surgical Section of the Royal Academy of Medicine in Ire-

land. Owing to the great mutilation and consequent lopsidedness, Messrs. Fannin made for me a very efficient prosthetic apparatus, which in addition to the æsthetic effect was useful in many ways. The manufacture of this took some considerable time, during which the patient remained in hospital. She was in capital health until the middle of February, when her temperature rose, she complained of pain in the right (opposite) side of the chest, had some cough and rusty sputum. I was very anxious lest these troubles were indicative of secondary lung infection. My colleague, Dr. Parsons, kindly saw the patient with me, and he was of opinion that pneumonia of the right lowest lobe had developed; this gradually cleared up and was followed by the occurrence of an area of dulness over the lower left lobe accompanied by fever. She had not completely recovered from this when she left the hospital three weeks later. I have heard from her at intervals since. She was in fairly good health for two or three months; however, the last time I heard of her (end of September) she was suffering great pain in the left side, had some cough and hæmoptysis, and was evidently dying of secondary lung trouble. There was no recurrence in the scar.

Examination of the recent specimen confirmed the conditions seen in the skiagram, the tumour was not encapsuled, and was traversed by easily broken down trabeculae of bone, which, however, did not show distinctly the radiating arrangement so often seen in these tumours. A macerated preparation which might have brought this out was not made.

An interesting point in the case is the history of injury. The "traumatic malignancy" of the older writers seems to have some foundation in fact: there certainly seems to be some relationship existing between previous injury and the development of sarcoma, particularly of bone. Some time previous to the admission of this case I had in my male ward a youth about twenty years of age, who stated that his knee was perfectly well until he injured it by falling into a ditch. He suffered from the immediate effects for about three weeks; after a temporary immunity of a month or so, he gradually "lost the use of his leg," and was admitted under my care five months later with a spindle-celled central sarcoma

of the head of his tibia, about the size of an average cocoanut. I do not wish to imply that I believe injury has any more effect than precipitating a latent condition.

The operation performed in the case of Maggie C— was that associated with the name of Paul Berger, who devised it and advocated it for all cases of malignant disease of the upper end of the humerus in preference to disarticulation at the shoulder-joint. The flaps were not those exactly described originally, but were somewhat modified to suit the exigencies of the case. Since its inception it has been adopted as a surgical measure in cases of advanced malignant disease of the breast, involving the axillary vessels, very large benign tumours about the shoulder, extreme cases of gangrene, tubercle and septic conditions, as well as extensive injury. Mr. Treves is reported to have made use of it in this last-mentioned connection in South Africa. The mortality in non-traumatic cases is stated to be 20 per cent., in traumatic 30½ per cent., but statistics in a case like the one described are of very little value, each case has to be judged on its own merits. The general condition of the patient, the distribution of the tumour, secondary dissemination, and the personal equation of the operator, are all such very variable factors that the figures quoted convey very little meaning. The risks of the operation are (a) sepsis; (b) shock; (c) hæmorrhage; and (d) entrance of air into the veins.

(a) Sepsis occurring in cases not infected prior to operation is nowadays unpardonable. Suppuration developing in a wound made through unbroken skin and uninfected tissues is evidence either of a want of knowledge of the principles of asepsis or of neglect on the part of the surgeon or those for whom he is responsible.

(b) The precautions taken against shock in this case were the following:—The temperature of the operating theatre was kept at about 70° F.; the table had a water-vascular system in which hot water was circulating; immediately before the operation was commenced, *i. e.* when the patient was anæsthetised, $\frac{1}{30}$ of a grain of strychnine was administered hypodermically,—I have much more faith in strychnine as a prophylactic than a curative agent in the treatment of shock;—proper control of hæmorrhage of the principal vessels

and immediate clipping of all bleeding points; a reverence for living protoplasm by avoiding superfluous handling of the tissues, especially the large nerves, which were cut quickly with a very sharp scalpel; no unnecessary delay; the patient was kept properly under the anæsthetic during the whole time. The alternating condition of partial with deep anæsthesia so characteristic of the amateur anæsthetist is in my opinion responsible for a good deal of the shock experienced by those patients not having the services of an expert. The administration of a nutrient enema containing brandy when the operation was concluded was the last therapeutic measure employed.

(c) The efficient control of hæmorrhage is one of the most important factors in the prevention of shock. No Esmarch's bandage was used in case malignant emboli might be forced into the circulation; the limb was rendered "bloodless" by elevation. I have never used Esmarch's bandage in any case in the way it was originally intended to be used, but have found it convenient as a tourniquet in some amputations of the arm and forearm. The chief point in Berger's operation is the early ligation of the main vessels, thus preventing subsequent hæmorrhage. It is important, as Treves has pointed out, to tie the artery before the vein, thereby diminishing the amount of blood in the limb. No ligatures were necessary except for the subclavian artery and vein and the supra-scapular artery; the posterior scapular artery was not cut except in its ultimate ramifications in the muscles along the vertebral border of the scapula. I am not in favour of ligating all small bleeding points, as it further damages the tissues and leaves numerous foreign bodies in the wound; pressure forceps left on for a few minutes and slight torsion are quite sufficient. I reckoned on the pressure of the flaps against the chest wall by the dressings to prevent oozing, and so did not drain, but it was not quite sufficient. In a future similar case I would insert a gauze and wick drain for forty-eight hours or so.

(d) The risk of entrance of air into the veins is reduced to a minimum by the early ligation of the subclavian vein. This is one of the strongest arguments in favour of this particular method of removing the upper limb and the greater part of its girdle.

[The patient has died since the above was put in type of secondary lung infection.]

REMARKS

ON A

**CASE OF CEREBRAL TUMOUR
WITH OPTIC NEURITIS.***By **ERNEST CLARKE, M.D., F.R.C.S.**

LAST October I was consulted by a Mr. M—, complaining of defective vision in the right eye, and who thought he wanted a change of glasses. I found that he could barely count fingers at half a metre with the right eye, but the left vision was normal. The ophthalmoscope revealed post-neuritic atrophy in the right eye, and to my surprise, the good eye showed a "choked disc," the swelling amounting to 3 D. On going carefully into his history I learnt that he had been ill for about a year, but thought that he was now convalescent. The previous October he had a peculiar aphasic attack; he was giving a music lesson and suddenly lost the power of forming words, although he was conscious and could make a senseless guttural sound. This attack, which lasted only a few minutes, was followed by a definite nervous illness which was marked by languor, more or less headache, and on several occasions by a slight attack of sickness. About Christmas he consulted an eminent nerve specialist, who considered he was suffering from "petit mal," and after most careful examination could find no evidence of any nerve lesion. Tremors on the right side then made their appearance, most marked in the right hand at first, later in the right leg; he had several attacks of numbness of the right leg, and at longish intervals difficulty of speech resembling the attack that ushered in the illness. In May he saw an oculist, who prescribed glasses but found no eye lesion. In July he went to Penmaenmawr for a month's holiday, and while there lost his headache. He first noticed the defective vision of right eye in Wales. There was no history of syphilis, or injury to the head, and previous to this illness he had enjoyed fairly good health. He was thirty-six years of age, had

been married some years but had no children. On examination, marked tremors of the right limbs chiefly noticeable in the right hand were apparent; these tremors were increased when he directed his attention to them or by effort tried to stop them. Reflexes were exaggerated, but not more than one sees sometimes in normal cases. He had a slight slowness of speech, but nothing approaching aphasia. He was perfectly intelligent and answered all questions correctly, but when not addressed did not volunteer any remarks, and lapsed into a listless or apathetic condition. He was well nourished, and his heart, lungs, and abdominal organs were reported normal. He had a fair appetite and seemed to the casual observer to be in very fair health.

Under homatropine I made a more careful examination of the eyes. The right showed the characteristic post-neuritic atrophy, a clearly defined greyish-white disc, with attenuated arteries. The left showed one of the best examples of "choked disc" I have ever seen, the swelling amounted to about three dioptries, and the vessels which were engorged and tortuous dipped down from the swollen papilla in a very marked manner. There were no hæmorrhages, and no retinitis with the exception of some slight infiltration in the immediate neighbourhood of the disc. I came to the conclusion that the patient was suffering from a tumour of the left frontal lobe, limited behind by the ascending parietal convolution (the leg- and arm-centre), and limited below by the third frontal convolution or speech-centre, the growth impinging on these parts and thus causing irritation manifested in the tremors, but not as yet implicating them sufficiently to cause paralysis.

Mr. Arbuthnot Lane saw the patient with me and agreed in the diagnosis. We both felt that opening the skull, if only to relieve the pressure, was urgent, but that as the patient was so well it would be expedient to watch him for a few days before operating. This was on the Saturday, and he went into a home that afternoon. Mr. Lane and I saw him the next afternoon, Sunday, and found him very ill; he had been vomiting all day, and complained very much of his head, and it was decided to operate the next day. Monday morning he was much worse, and he was rapidly becoming comatose when placed on the operating table. Chloroform was administered by Mr.

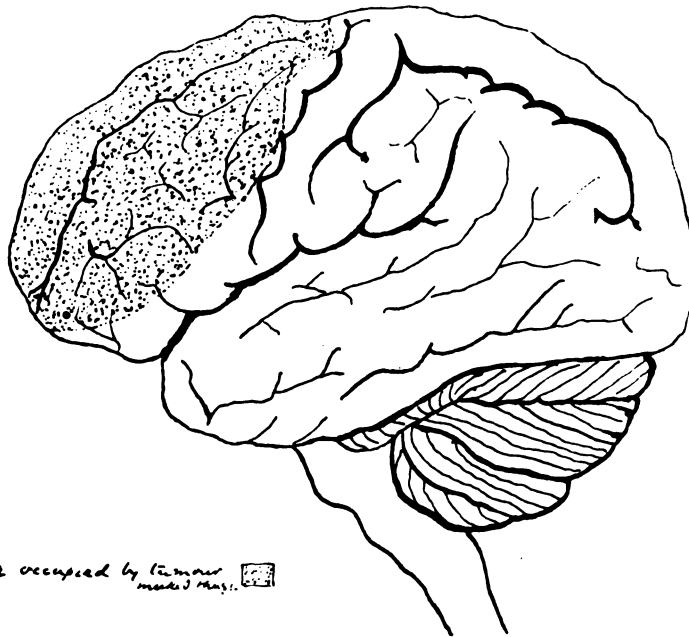
* Delivered at the British Medical Association Meeting, Ipswich, 1900.

Rowell. Mr. Lane made a large opening in the left parietal bone over the left ear. On opening the dura mater the brain bulged through, forming a lump the size of a child's fist. No tumour was discovered here and the soft parts were brought together and the patient put back to bed. He died early the next morning, and the same evening the brain was removed for examination. There was no meningitis. The frontal lobe on the left side was markedly different from the right; the portion of brain bound behind by the ascending parietal convolution and below by the third frontal was firm and hard, and

and hydrochloric acids. Under the microscope it shows no crystalline character.

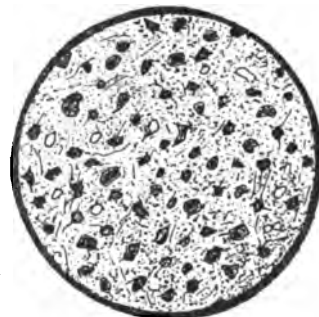


Tumour of brain showing vacuoles.



Area occupied by tumour marked May 1900.

resisted the impress of the finger. Both optic nerves were swollen, especially the left, and were removed for examination. On cutting through the tumour, numerous hard gritty particles, some as large as a big pin's head, were encountered. Microscopical examination of the growth shows hyperplasia of the connective-tissue stroma, atrophy of the ganglion cells, and an abundance of small round cells invading the whole structure, and in places a crowd of definite vacuoles, some containing pieces of "grit." The so-called "grit," which was in places so massed that it felt exactly like bone and turned the edge of the razor, is insoluble in strong and dilute nitric, sulphuric,



Section through tumour of brain.

The sections through the optic nerves show increased connective tissue, marked separation of the nerve bundles, and small round-cell infiltra-

tion. On the right side there is atrophy of the nerve elements.



Section through left optic nerve.

The special interest of the case lies in:—(1) The confirmation of the diagnosis, the tumour occupying exactly the situation mapped out during life; (2) The absence of serious symptoms until within forty-eight hours of death, the patient coming to me only for change of glasses; and lastly (3) The character of the tumour.

The presence of so large a growth without serious symptoms is probably explained by the fact that the area affected governs the higher mental faculties, and is supposed to be the only part of the brain that is not concerned with movement or sensation. The rapid onset of serious symptoms may have been merely a coincidence, or have been hastened by the mental anxiety connected with the preparations for his treatment. If one has to give the tumour a name, it is, I suppose, a psammo-glioma. The "grit" in psammoma tumours is carbonate of lime, which is certainly not the case here, and I do not yet know what the substance is.

The condition of the two optic nerves tends to show that both Manz and Leber and his pupil Deutschmann may be correct in their views as to the cause of the "choked disc." Manz maintains that it is due to hydrops of the intervaginal space—the mechanical theory, while Leber and Deutschmann think there is not merely stasis, but an active inflammation caused by the passage of irritating substances from the cranial cavity down to the nerve-head—the inflammatory theory. The presence of normal vision with such a marked "choked disc" is explained by Leber on the ground that the nerve-fibres may accustom themselves to the increased pressure without impairment of vision if the change takes place slowly enough.

CYCLIC ALBUMINURIA.

BY

G. A. SUTHERLAND, M.D.,

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(Concluded from p. 400.)

DURATION AND PROGRESS.

The statistics available for a consideration of the progress and duration of this affection are not numerous, and various forms of intermittent albuminuria are classed together by some writers in a manner which renders them useless for our purpose. The following cases appear to belong to the class of pure cyclic albuminuria.

Lacour had a patient who at the age of five years suffered from scarlet fever, and a year later was found to be the subject of pure cyclic albuminuria. This albuminuria persisted for seven years without leading to any degeneration of health or to any cardio-vascular changes.

Saundby has seen a case last eighteen years without developing further renal trouble.

Marie relates the case of a man aged thirty-two years, who had presented the symptoms of this affection for ten years, and was definitely known to have had albuminuria of this type for seven years without any change in the albuminuria, or the patient's general condition.

Teissier has reported a case of complete recovery in a woman ten years after the condition was first determined.

Herbert Hawkins has observed a case for five years without any change in the albuminuria, or any impairment of the patient's health.

Heubner has been able to follow up five of his cases of pure cyclic albuminuria to adult life.

(1) A girl aged eleven years when first seen was reported at the age of twenty-one to be in the best of health, but no examination of the urine was made.

(2) A girl aged fourteen years when first seen was in sound health at the age of twenty, and the urine had been free from albumin for some years.

(3) A girl aged twelve years when first seen was

in good health at the age of nineteen, and the urine had been free from albumin for three years.

(4) A girl aged ten years when first seen was found at the age of eighteen to be healthy, and without a trace of albuminuria.

(5) A boy aged thirteen years when first seen was in good health at the age of twenty-one. These were all regarded by Heubner as cases of pure cyclic albuminuria, and three of them had suffered previously from nephritis following scarlet fever or diphtheria.

The progress and duration of my own cases are shown in the accompanying table. Unfortunately the periods of observation are too short to allow of any final conclusions being drawn, but so far as

they go they indicate (1) a tendency towards improvement of the general health, and (2) that the supervention of evidence of organic disease is not to be expected.

As regards the occurrence of nephritis, there was no evidence at the final examinations of cardiovascular changes, or retinitis, or pronounced œdema, or persistent albuminuria, or polyuria, or the presence of tube-casts. While admitting fully the insidious nature of chronic nephritis, and the length of time which elapses in some cases before definite signs are present, it may be pointed out that in a series of fifteen cases of well-marked albuminuria a total absence of any such signs during prolonged observation is scarcely possible.

TABLE VII.—*Showing duration and progress of cyclic albuminuria in fifteen cases.*

Case.	Sex.	Age when first seen.	Period under observation.	Progress.
1	Male	9	4 years	Æt. 13 years. General health good; slight amount of albumin and nucleo-albumin in urine during the day; no cardio-vascular changes or other evidences of nephritis; spleen slightly enlarged.
2	Female	7	5½ years	Æt. 12½ years. General health unstable; subject to nervous attacks and abdominal pains; no evidences of nephritis; right kidney enlarged, movable, tender; cyclic albuminuria unchanged.
3	Male	10	1 year	Æt. 11 years. General health and cyclic albuminuria unchanged; no signs of nephritis.
4	Female	11	3½ years	Æt. 14½ years. General health good; morning urine no albumin; faint nucleo-albumin reaction; 3 hours later slight amount of albumin and nucleo-albumin. Menstruation began 1½ years ago; pigmentation and tachycardia present; no signs of nephritis; right kidney rather enlarged and movable.
5	Male	11	3 years	Æt. 14 years. Patient reported to be in good general health; is still nervous, and suffers from incontinence of urine at times; no examination made.
6	Female	12	3 years	Æt. 15 years. General health good; very neurotic; subject to erythematous eruptions. Menstruation began 2 years ago; examination of urine showed complete absence of albumin and nucleo albumin; no signs of renal disease.
7	"	7	6 months	Æt. 7½ years. No change in general condition or cyclic albuminuria.
8	Male	10	2 years	Æt. 12 years. General health good; albumin and nucleo-albumin during the day; no casts; no evidence of nephritis.
9	Female	12	3 years	Æt. 15 years. General health good; is anæmic. Menstruation began 1 year ago; no albumin or nucleo-albumin in the urine; no evidence of nephritis.
10	"	12½	6 months	Æt. 13 years. General condition and cyclic albuminuria unchanged.
11	Male	10	8 months	Æt. 11 years. General condition and cyclic albuminuria unchanged.
12	Female	9	1½ years	Æt. 10½ years. General health good; very nervous; cyclic albuminuria unchanged; puffiness and pigmentation of eyelids; fine tremors in hands and feet.
13	Male	10	4 months	Æt. 10 years. Disappearance of œdema of the eyes and headaches; general health improved; cyclic albuminuria unchanged.
14	Female	11	4 months	Æt. 11 years. General condition much improved under tonic and dietetic treatment; cyclic albuminuria unchanged.
15	"	12	2 months	Æt. 12 years. Patient continued languid, nervous, and irritable; pains and broken sleep recur at times; cyclic albuminuria unchanged.

In the majority of the cases the general health during the period of adolescence was not good, the chief symptoms being referable to an unstable condition of the nervous system. After the age of thirteen an improvement was sometimes observed, but in only two of the cases, both of them girls aged fifteen, had the cyclic albuminuria ceased. The establishment of menstruation is not necessarily followed by a cessation of the albuminuria. Some of the vaso-motor disturbances, such as flushing, heaving cardiac action, pulsating vessels, pigmentation, and puffiness of the eyes, were as pronounced at the final examination as when the patients were first seen.

There is little doubt that cyclic albuminuria is essentially an affection arising during early adolescence, and a study of my own cases and those of others has led me to regard the progress of any single case as uncertain, and to conclude that recovery, either as regards the albuminuria or the accompanying symptoms, is not to be expected during adolescent life. After the termination of adolescence the progress will probably be in one of the following directions: (1) Both the albuminuria and the symptoms of nervous disturbance may cease entirely. (2) The albuminuria may persist while the other symptoms subside. This is probably the condition met with frequently amongst young adults, the albuminuria being accidentally discovered, as at an examination for life insurance, etc., and the subject of it being described as apparently healthy. The duration of the albuminuria may in such cases be indefinitely prolonged. (3) Both the albuminuria and the other symptoms may persist indefinitely, but without evidence of organic disease. To the symptoms of vaso-motor disturbance present during adolescence may be added others, for example, those of Graves's disease as already described.

PROGNOSIS.

On this subject, also, there is a divergence of opinion, some writers holding that it is good, and others that it is bad, with a preponderance in favour of the former view. Pavy says it is important to distinguish the ordinary forms of albuminuria from cyclic albuminuria, as the gravity of the two is diametrically opposed. The latter may persist for years unaccompanied by any impairment of health, and there are none of the constitutional

indications of the existence of Bright's disease present. Dickinson states that after a longer or shorter time the albumin ceases to appear, and that the condition is not one of nephritis, although he cannot say precisely what it is. Amongst others who take a favourable view are Broadbent, Grainger Stewart, Saundby, Heubner, Marie, Lacour, Reckmann, and Holt. On the other hand, such cases were received by George Johnston with great suspicion. He said that the presence of albumin in the urine, although small in amount and occasionally intermittent, is always pathological. While admitting that an intermittent albuminuria might be cured by the removal of the exciting cause, he thought the neglect of treatment might convert an intermittent into a persistent albuminuria with fatal renal degeneration. Osler also holds that the presence of albumin in the urine in any form, and under any circumstances, may be regarded as indicative of change in the renal or glomerular epithelium. Vanderpoel states that his personal feeling towards cases of functional albuminuria is distrustful, and that only when the albumin had been absent for weeks or months consecutively, and the urine becomes entirely normal, would he relax that feeling. He does not consider that the natural tendency is in that direction, and therefore supervises the cases carefully, giving a guarded prognosis meanwhile.

The prognosis may be considered (1) as regards the prospect of future nephritis; (2) as regards admission to the public services and life assurance; and (3) as regards the patient's future health.

(1) Up to the present time no case of pure cyclic albuminuria terminating in nephritis has been recorded, and some cases have been under observation for periods of seven, ten, and even eighteen years. As far as can be gathered from clinical records it would appear that in some cases the albuminuria ceases entirely, while in others it persists unchanged for an indefinite period. Just as the onset of the condition is probably associated with the early changes of puberty, so its disappearance is frequently associated with the completion of that developmental period, which occurs in some much later than in others. An attack of acute infectious disease in a patient suffering from cyclic albuminuria is usually accompanied, not by an increase in the amount of albumin, but by its total disappearance, probably

from the fact that he is kept lying down in bed. This would tend to show that the vulnerability of the kidneys is not specially increased by the existence of cyclic albuminuria. Even those patients who have previously suffered from nephritis, following scarlet fever or diphtheria, do not show signs of its recrudescence during or after the existence of cyclic albuminuria. At present, therefore, there is no evidence that cyclic albuminuria is an early stage of nephritis, or that it predisposes to nephritis, but an absolute prognosis cannot be given until fuller clinical knowledge of the ultimate termination of such cases has been obtained.

(2) *Admission to the services and life assurance.*—

Sir Dyce Duckworth has laid down the following conditions with reference to accepting these patients for admission to the public services, namely, that it be distinctly shown that cyclic albuminuria is in no degree a sequel of scarlatina, that it is independent of structural renal degeneration, and unaccompanied by any cardio-arterial change, and that it passes off completely. These tests, although they may press hardly on individuals, and will probably lead to the rejection of almost all cases of cyclic albuminuria, seem very proper in the present state of our knowledge. Although a history of previous scarlet fever will only occasionally be present, and does not seem to have any bearing on the condition, and although evidences of structural renal degeneration and cardiovascular changes will be absent in purely cyclic albuminuria, it seems to be very rare for the albuminuria to cease while the patient is under observation, and it is very difficult to say when it will cease. Heubner's five cases, which were followed up and terminated in recovery, are instructive, but even here a doubt lingers as to the value of his statistics, because the fact of ultimate recovery was not ascertained in all cases by a complete medical examination. It would appear that adolescent albuminuria frequently persists until the age of twenty, and may persist much longer, so that the age limit of many of the public services would be passed before the affection had ceased. In addition to the prognosis from the examiner's point of view, which will naturally be a question of the albuminuria, the prognosis as regards the patient's general health in the public services must be considered by the family

physician. He will recognise the general nervous instability, and its various local manifestations, and will have to consider the importance of these in determining whether a life in the public services is or is not the best suited for his patient.

As regards life assurance, considering that we are not in a position to state definitely what the course of the affection is, and that we do not know the pathology, it would appear to be advisable to proceed very cautiously, and either defer acceptance until albumin has been entirely absent for a year, or regard the case as one of albuminuria of doubtful origin. In life assurance circles albuminuria has obtained such a definitely recognised position that we are not justified in altering established practice until our own knowledge is more complete than at present.

(3) As regards the patient's future health, I believe the prognosis to be good provided that his life is regulated according to the rules to be given later. There is no evidence of any one having died of cyclic albuminuria or of Bright's disease supervening upon it. There is evidence that some patients have presented the symptoms of this affection for many years without any deterioration of their general health. The prognosis, therefore, as regards the albuminuria, is good; but one has also to consider the general health. Since these patients are distinctly neurotic, they are liable to the perils of that class of sufferers, and under unsuitable surroundings as to work, diet, exercise, etc., may develop into hypochondriacs or neurasthenics. Under suitable surroundings and a careful mode of life, the general health will probably be quite up to the average.

TREATMENT.

The treatment may be considered as directed first to the cyclic albuminuria, and next to the patient's general condition.

I have tried various tests and drugs without any appreciable effect on the albuminuric condition. Exercise in the house or in the open air, running up and down stairs, cold baths night and morning, oxygen inhalations, and diet of the simplest or the fullest variety proved equally inefficacious. Massage of the abdomen and lower extremities, with the application of a tight abdominal bandage before rising, produced no alteration. Many drugs were employed with a purely negative result. Amongst these may be mentioned calomel, salicylate and

benzoate of soda, digitalis, strophanthus and nuxvomica, iodide of potash and nitro-glycerine, arsenic, iron, belladonna, pepsin, and supra-renal extract. These experiences agree with those of other observers, so that at present no method of directly controlling the albuminuria is known. Failure in this direction has naturally led to a consideration of the patient's general condition, and an attempt to improve whatever therein seemed faulty.

In the case of children admitted into hospital one was forcibly impressed by the rapid improvement which followed. The malaise and general pains and disturbed nights soon became things of the past without any active treatment, so that it seemed only natural to assume that the change to a quiet existence, with regular hours and a simple diet, was the main factor in producing this result. The benefits obtained in hospital were also continued at home when the parents were sensible enough to secure for the patients the same conditions of life. It seems advisable to recognise that these children, at an extremely critical developmental period, are suffering from nervous instability, and are not fitted for the ordinary life of other children. For the time being the important thing is to make them healthy animals. If they can be sent for a prolonged residence to some quiet part of the country in a dry lofty situation, where they can lead an out-of-door life, marked benefit may confidently be expected. Where this is not possible, the home life should be devoid of mental work or excitement, so that school, children's parties, exciting books, etc., ought to be stopped for a time. A full amount of sleep—ten to twelve hours—is very desirable, as many of these patients have extremely active minds, and have suffered from broken sleep. Under suitable surroundings hypnotics will not be required. The diet should be simple, and full, and regular, with meat in strict moderation, with as much fatty food as can be digested, and with an entire absence of highly spiced and indigestible materials. Flushing out the system by the addition of a pint and a half of plain water or barley water to that usually taken will remove the sluggishness of the excretory organs which is usually present. The tendency to periodic gastrointestinal attacks will be further lessened by a full dose of sulphate of magnesia or other saline twice a week. When the signs of nervous irritability

reach an acute stage, the condition already referred to as nerve-storms, the best treatment is complete rest in bed for some time, with low diet, and the administration of small doses of bromide of potassium and chloral hydrate. Dr. Mason of Maidenhead has informed me that in a well-marked case of such attacks, he found that small doses of bromide of potassium were most efficacious. Marie strongly recommends antipyrin in full doses at the beginning of an attack, and says that if given early enough it will cut it short. Teissier uses the bromides and quinine as part of the ordinary tonic treatment. Cod-liver oil and the hypophosphites are also valuable adjuvants in toning up the whole nervous system.

Remembering also that these children are neurotic themselves and of a neurotic parentage, we must not allow them to be treated as, or to develop into chronic invalids, to which condition many show a strong predisposition. It is questionable whether such patients should be sent to public schools, as the peculiar symptoms from which they suffer are not such as to provoke much sympathy from schoolfellows, and lasting harm may be done to the nervous system.

The condition of these patients after the period of puberty is over is one about which our knowledge is still extremely scanty. In some cases it would appear that the cyclic albuminuria ceases entirely, and the accompanying symptoms vanish, while in others the morbid state persists for an indefinite period. The treatment will therefore depend on the condition which is present when the patient attains to adult life. Certainly some of these cases continue in a state of nervous instability, and under the stress of mental worry or overwork are apt to break down. It will be advisable for them to select an occupation which is physical rather than mental, and to lead a life which is spent in the open air rather than one of a sedentary character.

MR. JOHN POLAND has brought up to date the second edition, now in the press, of vol. i of the late Mr. Chance's 'Bodily Deformities,' and is also engaged in editing the hitherto unpublished second volume, on the 'Treatment of Deformities,' by the same author. Messrs. Smith, Elder and Co., the publishers, have certainly done well in securing such valuable literary assistance, which goes so far towards ensuring complete success.

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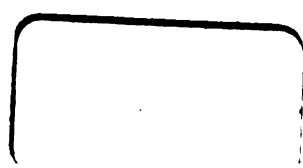
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